NAVAL PROFESSIONAL PAPERS

No. 16

ENGINES, BOILERS,

AND

TORPEDO BOATS

NAVAL PROFESSIONAL PAPERS.—No. 16.

PAPERS AND DISCUSSIONS

ON

ENGINES, BOILERS

AND

TORPEDO BOATS.

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NOTE.

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2

CONTENTS.

		Page.
I.	—On the Triple Expansive Engines of the Steamship Aberdeen. (2 plates.) By A. C. Kirk	5
II.	—On the Economy of Compound Engines. (2 plates.) By W. Parker.	11
	Discussion on two preceding papers. (1 plate.)	16
III.	—Lloyd's Rules as affecting Marine Boiler Construction. By J. T. Milton.	29
IV.	—On Corrosion in Steam Boilers. (1 plate.) By W. J. Norris	51
V.	—The Influence of the Board of Trade Rules for Boilers on the Commercial Marine. By J. T. Milton	63
VI.	—On Certain Points of Importance in the Construction of Ships of War. By Captain G. H. Noel, R. N.	93
VII.	—Efficiency of Guide Blade Propellers. (3 plates.) By J. I. Thornycroft.	117
VIII.	—On Sea-going Torpedo Boats. By J. A. Normand	131
IX.	—Some Experiments to test the Resistance of a first-class Torpedo Boat. (1 plate.) By A. F. Yarrow	143
X.	—Description of a Method of Investigation of Screw Propeller Efficiency. (4 plates.) By R. E. Froude	151
XI.	—On Steam Trials of the Satellite and Conqueror under forced Draught. (1 plate.) By R. J. Butler	179
XII.	—Advantages of increased Proportion of Beam to Length in Steamships. (2 plates.) By J. Harvard Biles	201
XIII.	—A self-propelling, self-careening Floating Dock. (1 plate.) By G. R. Rennie	213

I.

ON THE TRIPLE EXPANSIVE ENGINES OF THE STEAMSHIP ABERDEEN.

By A. C. Kirk, Esq., *Member of Council.*

[Read at the twenty-third session of the Institution of Naval Architects, 29th March, 1882; the Right Hon. the Earl of Ravensworth, president, in the chair.]

Economy of coal in our steamships is a point of so great importance, that any step in this direction is worth chronicling. Steamships are now making long voyages, and at high rates of speed, which, till within a few years ago, were made by sailing ships. To accomplish these profitably, economy of coal is all-important. Every ton of coal saved means a ton of freight earned, and in many trades every stoppage to coal is a loss of time, besides the exorbitant rates for coal charged at these ports. Coal-saving, too, is equally important for high-speed steamers, as, unless their voyage is only one of the shortest duration, coal becomes the greatest weight the ship is burdened with.

The obvious direction in which to look for saving is increase of pressure. Unfortunately, as we get higher in pressure, we do not by any means gain in efficiency in anything like a proportionate degree, and the time must come, may indeed not be far away, when further increase of pressure will not pay. This time has not come yet.

When James Watt utilized the lower pressures, from 15 pounds downwards, he unquestionably had at his disposal the most prolific portion. Pressure gradually rose—I am speaking now more particularly of the marine engine—to 30 pounds, and sometimes 40 pounds, but, used as the steam was, in one cylinder, exhausting direct to the condenser, little additional economy was gained by this increase of pressure. The variation in temperature, over 300 degrees in the cylinder, was too great; steam was condensed at the commencement of the stroke, when it had its full work before it, and re-evaporated towards the end, when it had little left to do but go straight to the condenser. Steam-jackets ameliorated this action, but besides the difficulty of getting them attended to at sea, they effected their purpose imperfectly and at a very considerable sacrifice of steam.

The next step was to divide the work between two cylinders, effecting part of the expansion of the steam first in one cylinder and completing it in a second, thus limiting the range of expansion and of tem-

perature in each cylinder. Several incidental advantages accompanied this change which we need not advert to here. A marked economy followed this improvement, and when a pressure of 60 pounds per square inch was reached, the economy over the older form was very marked; in round numbers, only about half the coal was required.

For six or seven years the pressure in use remained, for the most part, at 60 pounds, but of later years a gradual and steady increase of the working pressure of steam in our steamships has taken place, 100 pounds being now not uncommon. So far, however, as the imperfect data obtainable of steamship performances at sea show, the increase in economy of fuel has made but little progress, and I am not aware that any perceptible increase of economy has been attained by exceeding 70 pounds to 75 pounds pressure. In fact, the compound or double-expansion engine has relapsed into the condition of the old single-expansion one.

This gradual increase of pressure, using all the time the ordinary type of internally fired fire-tube boiler has dissipated at once the mistrust of boilers of larger diameters, and the craving for boilers composed of water tubes and such other complicated arrangements. These may yet have their day.

However, I am indebted to one of these water-tube boilers for having driven me seriously to take up the question of utilizing advantageously steam of much higher pressure than was at that time generally in use. While I was with Messrs. John Elder & Co., in 1874, Mr. W. H. Dixon, of Liverpool, anxious to attain greater economy of fuel, made up his mind to fit his steamer Propontis with high-pressure water-tube boilers on Messrs. Rowan & Horton's patent, and thus I had to consider the best engine to utilize this high-pressure steam advantageously.

Being thoroughly convinced that the great secret of success in the ordinary compound engine of the day over the earlier simple engine (even the Woolf engine) lay in the range of temperature through which the steam in any one cylinder passed in the course of one stroke, being very much reduced (nearly halved in fact, compared with a single cylinder), it seemed to me that with these high pressures we must use three successive expansions, and divide the total range of temperature into three parts. Of course this was incidentally favorable to a more uniform distribution of strains, reduced leakage to the condenser, &c.

Thus, the engines of the Propontis, constructed in 1874 for a steam pressure of 150 pounds per square inch, consisted of three cylinders of progressive capacities, the smallest being the high pressure, to which the steam was first admitted; next the intermediate one, to which the steam passed from the high-pressure cylinder; and the third the low-pressure cylinder, receiving the steam from the intermediate cylinder and discharging it into the condenser. The arrangement consisted of a three-throw crank, with a cylinder above each, and possessed no specialities of construction or design.

Unfortunately, the boiler very early gave trouble and ultimately was taken out, the causes of which do not come within the scope of this paper; but during the time it worked at its full pressure I found, on comparing the diagrams with those of an ordinary compound engine, that these engines ought to have required only about 1½ pounds of coal per indicated horse-power. The practical results of working the engines were satisfactory, no difficulties being introduced by the use of high-pressure steam; indeed, they are at work to the present day, though at reduced pressure. For when Mr. Dixon took out the water-tube boilers the idea still haunted most people that an internally fired boiler was unfit for such high pressures, and the new boilers of the ordinary type only carried 90 pounds—still a high pressure for such a boiler in the year 1876. These engines are still doing good work in the Propontis.

From that time till January last year (1881), when Messrs. George Thompson & Co. intrusted to my firm (Messrs. R. Napier & Sons) the building of the steamship Aberdeen, I failed to find any one who cared to make so long a step, and in doing so I hope Messrs. Thompson will have their reward.

In designing a ship for the long voyage their ships make from this country to Australia and China, more importance attaches to a small consumption of coal than in ships making shorter voyages, and it was necessary to use every device to this end.

The engines of the Aberdeen are essentially of the same design as those of the Propontis, the cylinders being 30 inches, 45 inches, and 70 inches by 4 feet 6 inches stroke. The boilers, two in number, are ordinary double-ended boilers, constructed entirely of steel, with six of Fox's corrugated furnaces in each, the total heating surface being 7,128 square feet.

There is no superheater. The construction of these boilers for so high a pressure—125 pounds per square inch—was facilitated by their being built of steel and to Lloyd's, whose rules allow the shells to be made thinner than required by the Board of Trade, although the internal parts are as strong as those required by the latter. After all, the shell is the simplest and strongest part of a round boiler, where, even if built to Lloyd's, there is superabundance of strength, but to doubly insure success—the internal parts of a boiler being those which oftenest give trouble—they were made stronger than required by either Lloyd's or the Board of Trade, whose scantlings for these parts are practically the same.

The high-pressure cylinder was not jacketed, the second was jacketed with steam of 50 pounds pressure, and the low-pressure one with steam of 15 pounds above the atmosphere.

The Aberdeen is a ship built of iron, both ship and engines being to the highest class at Lloyd's, 350 by 44 by 33 feet. When the ship was complete, 2,000 tons of dead weight were put on board, and ar-

rangements were made to test the consumption on a six hours' run at 1,800-horse power; this, however, by the owners' desire, was reduced to four only. The coal was Penrikyber Welsh coal, and Messrs. Parker & Dunlop, who happened to be on board, kindly undertook to examine the state of the fires and see the coal weighed. The result was a consumption of 1.28 pounds per indicated horse-power. According to usual analogy, we should expect from this a sea consumption of good Welsh coal of from 1.5 to 1.6 pounds per indicated horse power. The next trial was to find the maximum speed, which, on four runs on the measured mile (occupying two hours), was 13.74 knots, the mean power being 2,631, and the consumption of coal during these two hours being 1 ton 17 cwt. per hour. I am aware that the consumption of coal on so short a trial is not of implicit value, but the trial was made with the utmost care, and is thoroughly reliable so far as it goes. The engine, I ought to mention, is fitted with "Weir's" feed-heater, with a view to the better preservation of the boilers.

I mentioned above that the high-pressure cylinder was not steam-jacketed, while the others were. As it did not seem to me that the value of jackets could be great in such an engine, with so limited a range of temperature in any one cylinder and a considerable speed of piston, I sacrificed the steam jacket in the case of the high-pressure cylinder, purely from prudential motives (the voyage being a very long one), to avoid any chance of cutting, when worked with very little lubrication, as it was desirable the engine should. Further, when we take into account the thickness of the interior chamber of the cylinder and the speed of piston, it is not conceivable that the interior surface of the chamber can be maintained by the steam on the outside of it, at a uniform temperature. In fact, the slower an engine runs the more efficient the jacket becomes.

There is no doubt that in such an engine as the above, or, indeed, in any compound engine when the range of expansion and temperature is kept within moderate limits, that economy from the use of steam jackets is at least small; that, in fact, the steam condensed in the jackets, if admitted to the high-pressure cylinder, would have been for all practical purposes as efficient.

This has been a fortunate thing for the compound marine engine, as it is always extremely doubtful if, at sea, the steam jackets receive the attention they require to make them efficient.

These remarks do not apply when engines are being worked at comparatively low powers.

I may mention that the weight of steam condensed in the jackets, carefully measured into a tank, was 3¾ per cent. of the greatest weight of steam admitted to the high-pressure cylinder (by diagrams, Plate I), the pressure on the jacket of the middle cylinder being 30 pounds, and on the low-pressure cylinder 10 pounds.

In a second experiment, the condensed water was still the same percentage when the pressure in each jacket was doubled.

I may mention that the loss of steam from the high-pressure cylinder to the low pressure, just before release, plus the steam condensed in the jackets, was the same as took place inside the cylinders with the steam shut off from the jackets. I do not, however, quote this as absolutely conclusive, for, by an omission, Weir's feed-heater was connected and at work on both trials, abstracting a certain amount of steam from the low-pressure receiver. Still, I feel certain the result is tolerably correct, as the quantity abstracted by the heater must have been very nearly in the same proportion on both occasions. But a much more conclusive argument is to be found in the trials of a compound engine at Blackburn, made by Mr. Longridge (see Engineering, February 24 1882), where it will be found that the feed-water per indicated horse-power per hour was, when no steam was in the jackets, 16.87 pounds, and when all the jackets were supplied with steam, 17 pounds; on a second trial the figures were respectively 16.97 pounds and 17.16 pounds.

Unfortunately, at sea, it is extremely difficult to get data of this sort with the completeness Mr. Longridge was able to carry out on land.

I made complete arrangements for some further experiments on the way from Glasgow to London, but, owing to the state of the weather, my good intentions, to a great extent, fell through.

The form of engine adopted on board the Aberdeen, of three cylinders, each over a crank, as in Fig. 1 (Plate II), though a very convenient form for overhauling and giving a very uniform rotary motion, is not, by any means, the only form in which such an engine can be arranged. Indeed, from the unwieldy dimensions the low-pressure cylinder would attain, it would not be advisable for large powers.

About four years ago—but several years subsequent to the Propontis—Messrs. Douglas and Grant, of Kirkcaldy, made a comparatively small set of marine engines for the Isa with triple expansion, by placing the first, or high-pressure, above what in an ordinary two-cylinder compound engine would be the high-pressure cylinder, as in Fig. 2 (Plate II). This makes a neat and, in some cases, a convenient arrangement but is open to the objection that, if you make the ratios of expansion approximately equal in each cylinder, the strains are very unequal, as also the several ranges of temperature. A better arrangement is, to have two low-pressure cylinders, with the high-pressure cylinder on top of one, and the intermediate cylinder on the top of the other, as in Fig. 3 (Plate II).

This arrangement is adapted to high powers, and does not occupy any excessive length. It is also the best plan for altering a compound engine of the present type into a triple expansive engine, as it does not alter the strains on the cranks, connecting rods, &c.

It farther lends itself to certain horizontal arrangements of engines, as are used in unarmored ships of war. The weight of the machine would not be perceptibly increased, while the weight of coal to be carried would be considerably reduced.

Engines of the S.S. "Aberd

Feb.Y 1882.
Full lines.
Dotted lines. } Feed Heater on.

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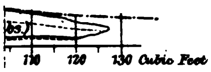

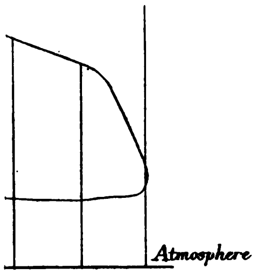

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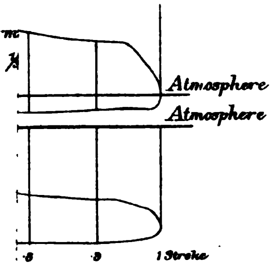

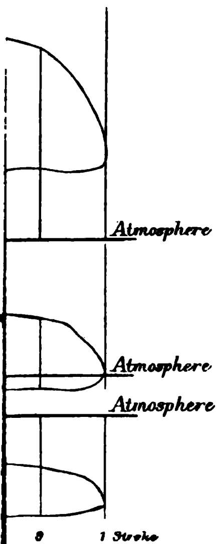

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II.

ON THE ECONOMY OF COMPOUND ENGINES.

By W. PARKER, Esq., *Chief Engineer Surveyor of Lloyd's Register; Member of Council.*

[Read at the Twenty-third Session of the Institution of Naval Architects, 29th March, 1882; the Right Hon. the Earl of Ravensworth, president, in the chair.]

During the last few years I have watched with great interest the continued increase of the steam pressures in the boilers of our merchant ships, first from 60 pounds per square inch to 70 pounds, and then to 80 pounds, and there is no doubt that an appreciable gain in economy of fuel has resulted therefrom. At present, very few engines are being made to work at a lower pressure than 80 pounds, while 90 pounds appears to be the more common pressure in use; still, I am almost inclined to doubt if sufficient economy of fuel can be realized by going above 80 or 90 pounds per square inch with the two-cylinder type of engine now in use. Having lately been investigating this subject, I think that a short description of the results obtained may not be uninteresting to this meeting.

The reason why, in general, higher steam pressures are conducive to economy is that they render possible a greater measure of expansion, as the most part of the work is done without any further expenditure of heat than originally supplied in the steam before expansion; but there are limits beyond which expansion of steam is not beneficial, and it is to these limits that I wish to draw attention.

If we conceive a perfect gas originally at a high pressure, and at any temperature to expand, doing work, the work will be done at the expense of the heat of the gas, and its temperature will fall by an amount proportional to the work done during expansion, unless an equivalent amount of heat be added as expansion takes place. If this heat be added, the relation of the pressure and volume during expansion is such that their product at any time is constant; but if no heat is added during expansion the pressure falls much below that which it would have been according to this law.

In the case of steam we have a much more difficult matter, as any reduction of temperature below that due to the pressure causes immediate condensation. However, when the law by which the pressures and temperatures of steam vary is known, and also the total amount of heat necessary to produce steam of any given pressure, we are able to calculate the variations of pressure and volume of steam, if we know what

heat is given to or taken from it, and also what work is done by it during expansion.

For example, if we take a given volume of dry saturated steam at a pressure of 140 pounds per square inch (absolute), and let it expand during work, we shall require to add heat to it at every step of the expansion in order to prevent liquefaction taking place, and although heat is added its temperature falls.

This is represented in the diagram, Fig. 1 (Plate I), by the drawn line, the temperatures at the various pressures and volumes being marked in figures thereon.

If now we were to allow the steam to expand without adding heat to it, or taking heat from it, but doing work during expansion, we should have partial condensation taking place; the process being represented by the dotted line in the diagram Fig. 1, the horizontal distance between which and the full drawn line, at any pressure, representing the amount of condensation which has taken place.

If now we could by any possibility allow steam to expand thus, say by using a perfectly non-conducting cylinder, we should get the maximum of work possible out of the steam, but, as I have stated, condensation must then take place. In practice, however, we are compelled to use cylinders of metal, the inner surfaces of which must be polished, and of course polished metal is a very superior conductor of heat; the inner surface of the cylinder must therefore always be tending to become of the same temperature as the steam immediately in contact with it. When it is colder than the steam it rapidly condenses part of it, absorbing its latent heat until its increased temperature is equal to that of the steam, while if the temperature of the steam is less than that of the cylinder, the surplus heat of the cylinder will by evaporating any condensed moisture on its surface rapidly lose heat until equilibrium of temperature again results.

This is the process which must take place in a cylinder at every stroke if the piston moved sufficiently slow; but even with a fast-moving engine the temperature of the inner surface of the cylinder will always be somewhere between that of the steam at entry and exit.

When, therefore, the steam first enters any cylinder its temperature is reduced by contact with the cylinder, and a certain amount is condensed. The effect of this is to warm the walls of the cylinder and the piston; as expansion takes place the temperature of the steam falls until it becomes lower than that of the cylinder, and then the hotter cylinder re-evaporates part of the condensed steam. Again, when the exhaust opens, a further reduction of pressure takes place, as it escapes into either a receiver or a condenser, and thus the re-evaporation becomes complete.

On fresh steam entering, as much of this is condensed as is required to raise the temperature of the cylinder to that of the entering steam. The effect of this whole operation is to admit a larger quantity of steam

from the boiler than ought to be required to fill the cylinder. It will thus be seen that in an actual engine the expanding steam must be having heat abstracted from it during a part of the forward stroke by the cylinder walls, in addition to that converted into work, whilst an amount of heat is added to it during the latter part of the same stroke, and during the whole of the return stroke, in re-evaporating the condensed steam.

The portion returned to the steam during the forward stroke helps to do some useful work by increasing the forward pressure, but the portion added to the steam during exhaust (really by increasing the back pressure) not only is directly wasted, but is actually creating resistance.

It is therefore evident that the greater the amount of expansion the greater will be the range of temperature, and the greater the range of temperature the greater will also be the amount of liquefaction and re-evaporation taking place, so that a point is reached at which with further expansion the greater variation of temperature causes greater losses of heat than is compensated for by the additional work done.

Now, provided we can expand the steam continuously, allowing the pressure to fall only as work is done, it is immaterial whether the expansion is effected in one or in more cylinders.

In the compound engine as in general use, however, this expansion is somewhat interrupted; the steam after it is cut off in the high-pressure cylinder expands until it fills this cylinder, and at the end of the stroke it expands further, without doing work, into the receiver. This represents a loss compared with what would have been realized by continuous expansion, and, in addition, the steam having to pass through tortuous passages in obtaining its exit from the high-pressure cylinder into the receiver, and from the receiver into the low-pressure cylinder, a loss of direct pressure is experienced, the forward pressure in the low-pressure cylinder when this is fully open to the receiver being generally two or more pounds less than the back pressure in the high-pressure cylinder.

These losses of power are represented graphically by the gap between the high and low pressure diagrams when expanded, as shown in Figs. 1, 2, and 3 (Plates I and II), and the continuous expansion curve.

Although a very considerable loss may thus be shown to take place in the best designed compound engine, yet the fact has been proved beyond question that the compound engine, even with this loss, is more economical by far than the simple engine; and the reason it is so is because, by expanding the steam into two cylinders, the range of temperature becomes divided into two, and in neither cylinder does such a great amount of condensation take place.

For instance, with steam of 60 pounds per square inch above the atmosphere, expanding to say 10 pounds absolute, or 5 pounds below the atmosphere, a single cylinder will be exposed to a range of temperature

from 307° to 194° during the forward stroke, and then to 100° during the return stroke, while in the compound engine the ranges will be from 307° to about 215° in the high-pressure cylinder, and from 215° to 100° in the low.

In an ordinary compound engine, as we increase the pressure, so also do we at the same time increase the range of temperature; and further, unless the high pressure cylinder is made unduly large, so that a long range of expansion takes place in it, a large amount of unbalanced expansion takes place from the high-pressure cylinder to the receiver.

Thus in Fig. 3 (steamship Northern) we see a great gap between the diagrams and the curve, so that, together with increased condensation, a limit must be reached in which further expansion in the two-cylinder engine will produce more losses from these causes than the gain from the additional expansion, exactly in the same way as with the simple engine a limit was soon reached beyond which additional expansion was injurious.

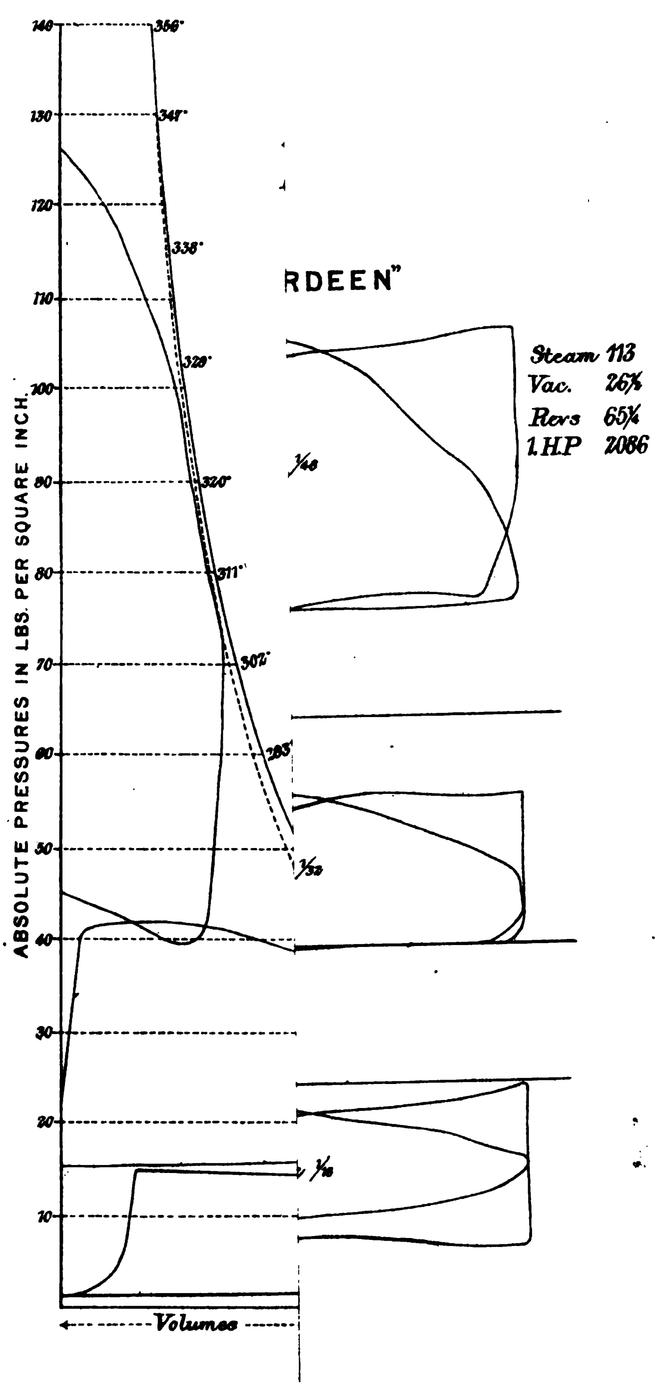
In other words, if pressures and expansions are carried beyond this limit, we shall have to again compound our compound engines.

Where this limit is must be ascertained by direct experiments, and I am sure that the engineering world are looking forward with intense interest to the results which will be obtained by the steamship Aberdeen in her forthcoming voyage as an experimental fact towards settling this point.

Unfortunately, with marine engines, it is almost impossible to determine their absolute efficiency; the only facts as to their performance which can be accurately noted are the indicated horse power and the consumption of fuel; this gives a measure of the efficiency of the engine and boiler combined, but in order to eliminate the boiler we would require to know in addition the amount of water evaporated, and also the amount of condensation water and its rise of temperature (this would enable us to know how much water was in the steam when it enters the engine, and also at any subsequent part of the stroke), but these quantities are not measurable in engines of large power, such as are fitted to the ordinary steamers employed in the mercantile marine.

Although it is impossible from the indicator diagrams to ascertain how much condensation takes place, yet on carefully analyzing them the re-evaporation during the latter part of the stroke can easily be seen, but that which takes place when the cylinder is open to the condenser cannot of course be ascertained in this way. If the clearance spaces of each cylinder be accurately measured, and the actual volumes of the steam at each pressure be set off in a diagram, as shown in the Figs. 1, 2, and 3, it will be found, I believe, in every case, that after the cut-off the curve formed will rise higher than the adiabatic curve through the cut-off point, showing that re-evaporation does take place, while in all cases the low-pressure diagram will fall below the adiabatic curve drawn through the fullest point of the high-pressure diagram, further showing

Plate I.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Steam 113
Vac. 26⅞
Revs 65¼
I.H.P 2086
ABSOLUTE PRESSURES IN LBS. PER SQUARE INCH.
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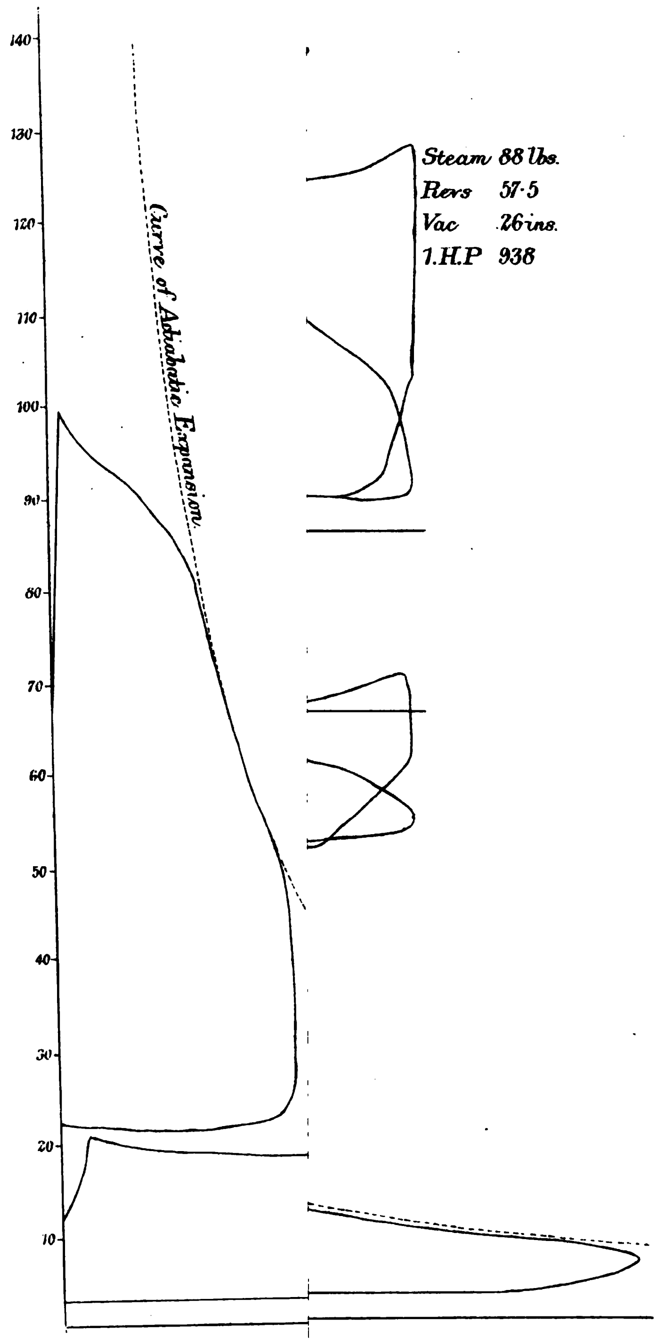
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10
Curve of Adiabatic Expansion.
Steam 88 lbs.
Revs 57·5
Vac 26 ins.
I.H.P 938

that some portion of the steam must pass through this cylinder as water, never appearing in the indicator diagram at all being re-evaporated during the return stroke directly into the condenser.

I have constructed diagrams (Plate I) showing the relative volumes and pressures of steam in the case of the steamship Aberdeen, taken from indicator diagrams, kindly given to me by Mr. Kirk, from the performance of that vessel at the power at which she will work at sea, and also from diagrams taken from two two-cylinder compound engines, one of them working at the same boiler pressure, viz, 125 pounds per square inch, and the other at 88 pounds per square inch.

It will be observed that in the latter case a considerable departure is visible in the high-pressure diagram from the adiabatic curve (or curve of perfect engine), while very considerable condensation is shown in the low-pressure cylinder, more than that in the high-pressure, and considering the rapid rate of evaporation in the high-pressure cylinder towards the end of the stroke, I believe that a very considerable amount of the steam must even then be left condensed.

In the first two diagrams of the steamship Aberdeen very little evaporation is shown, and this is what we should have expected from the small range of temperature in them.

Again, in the case of the engines of the steamship Northern, Fig. 2 (Plate II) being ordinary compound engines working at a pressure of 88 pounds per square inch, although the compression parts of the diagram clearly show that the high-pressure piston is leaky, yet the pressure in this case towards the end of the stroke rises above the adiabatic curve, while very considerable condensation has taken place in the low-pressure cylinder, and had the piston been tight these would of course have been more marked.

I think we may assume that the nearer the expansion curve in practice approaches that due to the perfect engine, the better will be the performance of the engine, and from these diagrams I have formed the opinion that the limit of beneficial expansion with the two-cylinder compound engine is about reached at a pressure of between 80 and 100 pounds per square inch, and that in order to obtain better results with steam above that pressure we must compound our present compound engines.

I may mention that within the last few weeks there has been a steamer completed to work at a pressure of 150 pounds per square inch, with triple expansive engines very similar to those fitted in the steamship Aberdeen, and in my opinion this description of engine marks one of the most important of recent advances in marine engineering, affording a means of using steam with economy at higher pressures than have hitherto been possible.

DISCUSSION ON THE TWO PRECEDING PAPERS.

Mr. William Denny. My lord, we have not as yet made any experiments on triple-expansive engines, but in the Watt lecture, which I delivered in January, I stated we were going to make experiments in a small steam-tender we were building for ourselves. We are building her with compound engines, but with a boiler of sufficient strength to stand 120 pounds pressure. We are going to try these engines with 90 pounds pressure, and we propose afterwards putting on, in the style of that No. 2 engine, the invention of Mr. Taylor, of Newcastle, to whom you referred this morning, a third cylinder, for the purpose of making them into a triple-expansion engine. We shall then raise the boiler pressure to 120 pounds, and try what the result is. When we have done that my firm no doubt will be very pleased, if the Institution cares to receive it, to give the result of those trials.

Mr. Longridge. My lord, the papers just read bring before us several points on which I should like to have spoken, but as time is getting short I will limit myself to one only, namely, the extent to which economy of steam depends upon the number of cylinders used. Mr. Kirk's paper contains two propositions: The first, that increased economy must be got by higher pressure; the second, that to make the economy due to higher pressure practically available we must increase the number of cylinders in which the steam is worked. The first I am willing to admit, though I believe the gain now possible to be only small; with the second I cannot agree. In support of it Mr. Kirk asserts that when boiler pressures were raised from 15 pounds to 60 pounds, and simple engines replaced by compound, half the coal was saved, and his conclusion is that the saving resulted from compounding; mine is that it resulted from the higher pressure. The old single-cylinder engine, working with a boiler pressure of 15 pounds, was practically a non-expansive engine; in the nature of things it could not be otherwise, for if the steam had been cut off early the back pressure would have increased so much relatively to the mean pressure as to neutralize the economy due to the higher ratio of expansion. The compound engine, working with a boiler pressure of 60 pounds, was, on the contrary, an expansive engine working under fairly economical conditions, for the higher boiler pressure permitted a fair ratio of expansion concurrently with a fairly high mean pressure, while the addition of the second cylinder allowed this higher ratio of expansion to be got conveniently with a common slide-valve. The same economy would have been got by expanding steam of 60 pounds in a single cylinder under proper conditions. The increase in the pressure was the primary and essential

cause of the economy; the addition of the second cylinder was only a convenient plan for getting the necessary number of expansions with a familiar valve-gear. In support of this assertion I might bring *a priori* arguments of equal force to those brought against it in the paper. Compare, for instance, the three-cylinder engine of the Aberdeen with a simple engine developing 1,800 indicated horse-power, assuming in both cases a speed of 60 revolutions and a back pressure of 3½ pounds. The constant of the big cylinder of the triple-expansion engine would be 63, and the power required to expel the steam 225 indicated horse-power. With a single cylinder and a ratio of expansion as high even as 1:10, a diameter of 59 inches would be sufficient to develop 1,800 indicated horse-power, and in this case the work of the expulsion would be only 154 indicated horse-power, or 70 indicated horse-power less than before; a gain of 3½ per cent. at once. Look at the matter from another point of view. What is it that prevents the practical realization of the economy promised by the theory of expansion? It is condensation; not the condensation throughout the mass of steam due to the performance of mechanical work, but the condensation due to the abstraction of heat by conduction from that part of the vapor which touches the metallic surfaces of the cylinders. The surfaces are, in the triple-expansion engine, 241 square feet; in the single engine 120 square feet, without counting the surface in the passages and ports, which of course is greater in the compound than in the simple engine. Does it not seem curious, on the face of it, to try to prevent condensation by doubling the surface causing condensation? Of course, by increasing the surface we reduce the difference of temperature, upon which also the flow of heat depends, and by so doing we get a certain proportion of the work done before the most serious condensation takes place; and this is absolutely all that can be said in favor of the compound engine. Is it sufficient to enable us to predicate of that engine any considerable economy, or any economy at all, as compared with the simple engine? I think not. But *a priori* reasoning about steam-engines is fallacious. Experiment is the only certain guide; and even experiments may mislead, unless the circumstances under which they are made are properly chosen. For instance, Mr. Denny has proposed to put the question of two cylinders *versus* three to a practical test by fitting an engine with two cylinders working at 90 pounds against an engine with three cylinders working at 120 pounds. This is not a proper test at all. What should be done is to try two cylinders against three with the same boiler pressure after finding out the best ratio of expansion in each case, for experiment has shown that for each type of engine there exists a certain ratio of expansion at which the best result is got. A careful series of experiments has been made in France to determine these particular ratios of expansion. With jacketed cylinders and pressures approaching 60 pounds they are, for single cylinders, about seven expansions; for compound engines, with cylinders 1:2, about eight expansions; with cylinders 1:3,

about ten expansions; with cylinders 1:5, about fifteen expansions. It is also shown that all the best results are practically the same, the consumption with about 60 pounds of boiler pressure being approximately 18 pounds of water per horse-power. One of my own experiments made under the same circumstances gave the same result. At 80 pounds pressure I have found the consumption with two types of engine to be nearly 16¾ pounds, and this is confirmed by one of the French experiments, before referred to. These figures do not justify us in asserting definitely that the consumption of all types of engines working at 80 pounds would be 16¾ pounds of water, but if we are to argue by analogy at all, then we should infer rather that it would be than that it would not. Both *a priori* reasoning and experiment, therefore, lead me to the conclusion that whatever other advantages the compound engine may confer, a marked economy of steam, such as is claimed in this paper, is not to be expected. My own experience leads me to think that the saving from increased pressure is roughly about 1 pound of water per horse-power for each 10 pounds rise in pressure at about 60 pounds, and somewhere about half as much in the neighborhood of 80 pounds, and less again for higher pressures. Besides increase of pressure there seems another way in which economy may possibly be effected, and that is by superheating. When I mention that with steam of 55 pounds pressure superheated 380° and expanded seven times in a single unjacketed cylinder a horse-power has been produced with 15½ pounds of water—a result little if at all inferior to that obtained from the engine described in the paper—I think I have said enough to show that the question of superheating is worth consideration. I should add that in the case referred to there was 24 per cent. of water in the cylinder at the end of the admission.

Mr. JOHN SCOTT. My lord, will you allow me, as an early worker in the introduction of the compound engine for marine purposes when that engine was not a fashionable engine, to say a few words on this very interesting question? I have listened with a great deal of pleasure to the papers which have been read by Mr. Kirk and Mr. Parker. I was one of those who commenced the introduction of the compound engine at the very early period of 1858. I commenced with the use of steam at 125 pounds, which Mr. Kirk has now been experimenting upon. I was obliged to abandon the use of that pressure because the boilers we then attempted to use, as happened with Mr. Kirk in 1874, boilers with internal water tubes, failed in continuous practice. Although after that period, in 1860, I recommenced with pressures of 60 pounds. I never lost faith in a pressure, for practical purposes, of 125 pounds, or even more. When I used that pressure in 1858 and 1860 (and possibly some gentleman may recollect it, or have seen accounts of those experiments in Professor Rankine's work on the steam-engine) only two cylinders were used in working that pressure of steam. If my recollection serves me right—and I am speaking entirely from memory about it—the results which

we obtained were not very different from those which Mr. Kirk has brought before us to-day. That system was abandoned at that time simply from the failure of the boiler, and not because we were disappointed with the result of the engines. Nobody, until Mr. Kirk led the way with this boiler, as fitted to the Aberdeen, has had the courage to use steam of 125 pounds at sea in a boiler of ordinary marine type, and I think Mr. Kirk deserves a great amount of credit for having been so bold as to do so. He has explained in his paper very well what has enabled him to obtain that result now, and I think a great deal of credit is indeed due to not only him, but to those who enabled him to carry out those experiments on such a large scale. Now, my lord, with regard to the curves on the diagrams here (Plate I, Mr. Kirk's paper), with reference to the question of the difference of economy to be expected between the use of three or four cylinders and two cylinders. In using an adiabatic curve, such as is shown on the diagrams, the measure of the loss caused either by condensation in the cylinder and re-evaporation, or any of the other losses, must be measured by the differences of area between the curves projected from the actual indicator diagram and that shown by the diagram. Now, in comparing the various curves produced by Mr. Parker and those produced in Mr. Kirk's paper, I must confess that, going roughly by the eye, I do not see any very large difference of area presented by those curves. It strikes me, as far as shown, the tandem engine of the Ludgate Hill, with two cylinders, does not very largely differ from the triple cylinder engine of the Aberdeen. I should have liked, if one or other of those gentlemen could have given us the exact figures as represented by the diagram they have produced, to show what those differences really are.

The PRESIDENT. They will be able to do that in their reply if they are furnished with them.

Mr. E. A. COWPER. My lord, I have drawn here two diagrams in order to try to show distinctly the difference between the area that can be obtained with 90 pounds steam in a compound engine, and what would be the effect with three cylinders, supposing the steam is 125 pounds to the inch. Fig. I (Plate appended to this discussion) is 90 pounds, and Fig. 2 is 125 pounds. Now, theoretically, Fig. 2 ought to be obtained by Mr. Kirk by going to 125 pounds pressure. I have not rounded off the corners so much as they would be rounded off in practice, because I wished to show, as nearly as I could, the true expansion curve—what the amount of power is that should be obtained in each case. Therefore, I should be very glad indeed, having had something to do with the compound engine, if Mr. Kirk can get that bit between the 90 pounds and the 125 pounds, but it must be admitted that it will be at the sacrifice of some convenience in the boiler, owing to extra stays and thick plates, &c. However, that is all that is on the cards, and no more is to be got than that. Now, taking the observations of Mr. Parker in reference to the steam being condensed, first in the one

cylinder and further on in the second cylinder, and passing out as water into the condenser, I must take exception to that statement. He did not state in the paper whether the cylinders were steam-jacketed. If that action took place they could not have been properly steam-jacketed. I drew attention to that effect, when I first introduced my arrangement for compound engines in 1857, and I afterwards improved my steam-jacketed reservoir in 1862. It was done in this way. There was a steam-jacketed reservoir, Figs. 4 or 5 (Plate appended), there was a lining inside L L open at one end, and closed at the other. As the steam passed through from the high-pressure cylinder at H, it passed into the middle of the space R, Figs. 4 or 5, without rubbing against the steam-jacket S at the side, and it did not increase the back pressure at all, and did not expand the steam at all, but when it passed out to the low-pressure cylinder at C, it passed round the edge of the lining L L through the passage P, and rubbed itself against the steam-jacket inside all the way round. I am afraid in many of the compound engines which are now made with steam-jacketed reservoirs, they do not put the linings L L in, and therefore they do not get the full advantage of superheating the steam when it passes out. The diagram Fig. 3 (Plate appended) shows the steam expanded as above-mentioned. The dotted line E E is the true expansive curve, and the full lines are the indicator figures. Instead of having any portion of the steam thrown away as water, as suggested by Mr. Kirk, we have the steam slightly superheated. It is not an experiment, but has happened over and over again; in fact, the steam-jacketing of the cylinder *ends* as well as the jacketings at the *sides*, and the jacketing in the *reservoir* "keep Jack alive" to that extent, that we get a good pressure right away out to the end of the low-pressure figure. In that way we can expand very many times, whereas in one cylinder there cannot be much more expansion than is shown by the dotted indicator figure O O. The consumption of fuel which Mr. Kirk has spoken of, 1.28, I am very glad to hear of, but it ought to have been better for 125 pounds pressure of steam, I myself having many years ago done 1.3 with 55 pounds steam. One reason is, in the Aberdeen, that he does not follow the expansive curve far enough—he should expand more in each cylinder. There is a large notch between each of those figures because the running of the curve is not followed. It does not expand enough in the high-pressure cylinder, or in the intermediate cylinder. Twelve per cent. ought to be got, and I need not mention that the difference between 1.3 and 1.28 is not anything like 12 per cent., but is only 1½ per cent.

Mr. Kirk. Would you kindly tell us by what kind of vessel the 1.3 was got?

Mr. Cowper. Her Majesty's ship Briton, 350 horse-power nominal working up to 2,100 horse-power, and also the Thetis, a sister ship.

Mr. Macfarlane Gray. The advantage in efficiency of increasing the pressure from the usual 60 pounds steam, that is 75 pounds gross,

to 124 pounds gross, theoretically amounts to about 16 per cent. The mean pressure during admission is, say, only 110 pounds (all the pressures will be given gross pressures), the gain would be, in that case, theoretically only 12½ per cent. I think that only about 9 per cent. increased efficiency can have been realized; that will amount to about 8¼ per cent. of fuel saved; but they say that 10 per cent. of the fuel is saved. The question is, is it worth while to add a cylinder and to, say, double the boiler pressure to save 10 per cent. of the present consumption of fuel? Thinking back upon previous advances in economy, we must, when judging of a percentage statement of saving by precedents, bear in mind that, at the beginning of compounding and surface condensation, 10 per cent. saved would be 10 per cent. on about 4 pounds per indicated horse-power, while it is now 10 per cent. upon only 2 pounds, or equal to what 5 per cent. was then. Ten per cent. less coal to be carried, even on the present diminished consumption, may be a sufficient inducement to adopt engines of this type for the China trade, in which the City of Aberdeen is to be employed, but on comparatively short voyages the increased first cost and increased tear and wear will, I think, more than counterbalance the gain.

In Mr. Parker's paper the process by which what is originally "dry saturated steam" does work is described. In my paper on thermodynamics two years ago I suggested that this term "saturated steam" should be given up as misleading. I consider that steam as obtained from the boiler is steam gas containing less or more moisture. The steam cannot be said to be "saturated" with moisture, for liquid particles continue to be formed in it during expansion. A perfect gas is matter traveling in single molecules; aggregations of molecules constitute liquid particles, each aggregation, however, still counting as one gas molecule, the additional molecules counting as liquid. This view of steam, which I am working out, has also since been advanced in an article by "Clausias" in the *Philosophical Magazine*, in June, 1880. The long time I have allowed to elapse between reading my paper and preparing it for the press is principally due to this, that although on every side I obtain remarkable corroborations of the soundness of my deductions, they, in respect to the varying specific heat of liquid water, to a small extent violate the second law of thermodynamics, while they agree with the first law. I am trying either to explain away that disagreement, or, otherwise, to heap up experimental corroborations to support my deductions in their violation of the second law, which, except for a perfect gas, is admittedly based on a mere assumption. What is called dry saturated steam at a given temperature is boiler steam at that temperature of maximum pressure with a supposed minimum amount of moisture. Otherwise it may be described as boiler steam at a given pressure at minimum temperature containing minimum moisture, or simply dry steam. In Regnault's experiments on latent heat, steam at 212° contained .991 of steam gas, and only .009 of moisture; that is the mean of forty-four ex-

periments at that temperature, but the proportion varies irregularly, and at the highest pressures experimented on, 200 pounds pressure, the moisture amounted to only 2 per cent. In Mr. Parker's paper the nineteenth formula approximation is employed. I make the decrease of the P. V. product to be more rapid in adiabatic expansion than that obtained by that formula. The following table gives some notion of the order in which condensation proceeds, beginning without moisture:

Pressure.	Steam gas.	Moisture.	P. V. new.	P. V.
Lbs.				
124	1,000	0	1,000	1,000
70	966	34	916	944
44	943	57	860	902
30	924	76	816	868
14.7	890	110	745	808
6.9	860	140	682	749
1.5	811	189	582	643

The column headed "P. V. new" is the P. V. according to my graphic method. The last column gives the P. V. by the formula employed for the adiabatic curve in Mr. Parker's paper. Either of these shows that the old rule P. V. constant cannot be applied when such a range of pressures has to be dealt with. If the intermediate steps between exhausts and inlets involve no loss of heat the P. V. products will be a little more than what are given in the second last column, which refers to perfectly adiabatic expansion. The adiabatic curve exhibited on a diagram at low pressures is not easily estimated by the eye, which can with difficulty take in, that a little up or down means a strip of area of that breadth the whole length of the diagram; where the curve is steep the differences are seen more clearly. With regard to the practical value of steam-jackets, I have frequently been told by sea-going engineers that they never put steam into the jacket except before taking an indicator diagram, because they cannot keep up steam with the jackets in use. With the steam-jackets they can get a prettier diagram. I do not mean to say that this is conclusive against jackets, because it is quite consistent with this statement that the speed of the vessel might be a shade more with the jackets under steam. The boilers, however, might not be sufficient to maintain the intended steam pressure with the jackets on, and that having to be logged the engineer would be blamed for not keeping up the pressure, and to escape blame he shuts the steam off the jacket. One type of engine illustrated, viz, that with the first cylinder tandem above the second cylinder, offers facility for greatly extending the life of the boilers. The top cylinder could be removed, and the engines worked at 60 pounds pressure. Boilers made originally for 120 pounds might, at the end of thirty years, if kept in repair, be still sufficient for 60 pounds.

Mr. RAVENHILL. I would like to ask Mr. Kirk one question; it would be very interesting if he could answer it; and that is, the relative weight of the machinery and water of the steamship Aberdeen compared to the ordinary form of compound engine of which he has made many, and where he has had a similar speed of piston. It would add greatly to the value of the paper if he could kindly favor us with that.

Mr. REYNOLDS. I venture to raise the question whether people can afford this kind of economy. I approach the subject with some diffidence, because I never heard two papers read which were less dogmatic, or which placed their views before us more lucidly and fairly. As far as they go they are convincing, that if we want to obtain any useful results from a higher rate of expansion than we have hitherto used, we must keep down the difference of temperature in each cylinder, and therefore must use more cylinders. But the question remains whether the economical result obtained is real, and shown in the driving of the ship, or whether it exists only on the indicator cards. With ordinary compound engines I take for granted, when I hear of a consumption of less than $1\frac{3}{4}$ pounds of coal per h. p. per hour, that the ship is doing very badly. Of course I do not argue that triple expansion may not get a stage further, and I think that it probably will do so; but I have this morning looked up a report in the *Times* of some trials made in August last of H. M. S. Nelson, a ship which has some very successful engines made under the direction of Mr. Kirk. I ought to say that on calling at the Admiralty I do not find this to be an official record, but it is certainly typical and instructive. The report appeared on August 22, 1881. I quote the figures only. With four boilers working, the revolutions per minute were 47, the h. p. 1,783.75, mean speed 6.95 knots, consumption of coal 1.7 pounds per h. p. per hour. With six boilers working, the revolutions were $63\frac{1}{2}$, the h. p. 3,067.01, mean speed, 10.6 knots, consumption, 1.9 pounds per h. p. With ten boilers working for six hours, the revolutions were 80, the h. p. 6,219.14, the speed 13.33 knots. Now it is obvious that at such moderate speeds the work done in driving the ship must be nearly in proportion to the cube of the speed, the circumstances being practically the same; and the total quantity of coal is found by multiplying the given horse-power by the rate of consumption given. Cubing the lowest speed and dividing the total weight of coal by the cubed speed, we get 9.03 pounds of coal per hour for each unit of cubed speed. But at the 10.6 knot speed, when the consumption has risen to 1.9 pounds of coal per h. p., the total coal divided by the cubed speed gives only 4.885 pounds of coal per unit of cubed speed. That is to say, the work done in driving the ship was, when burning 1.9 pounds of coal per h. p. per hour, nearly double as much as when burning 1.7 pounds of coal per h. p. per hour. At the maximum speed of 13.33 knots, when burning 2.2 pounds of coal per h. p. per hour, the consumption of coal per unit of cubed speed had risen to 5.77 pounds, that is to say, the point of maximum economy had

been passed, but still the actual result was 50 per cent. better than when the same engines were showing higher results upon the indicator cards.

Mr. JOY. Mr. Chairman, will you allow me to add a word or two on an experiment which was tried some six or seven years ago by the Barrow Shipbuilding Company in this direction, that is, with a special boiler to test the economy of a higher pressure of steam than was then generally used? At these experiments Mr. Parker was present, and will no doubt remember them. The steamer was fitted with boilers constructed to carry 120 pounds steam. The engines were compound, but had only two cylinders, one high-pressure and one low-pressure. After very carefully conducted trials, I was a little surprised to find that the consumption was as high as 1.67 pounds, while we learn that the Aberdeen with the same steam pressure has come as low as 1.3 pounds. It appears to me, then, that Mr. Kirk's and Mr. Parker's statement of the comparatively small variation of the temperature in the cylinders of the three-cylinder engine (so avoiding condensation) will account for the difference in the consumption of the two systems, and indeed will confirm the experiments. I have the indicator diagrams and all other particulars taken at the time, and these are at Mr. Parker's service, if he cares to compare them with those of the Aberdeen, as it might be interesting to note if the calculated loss by condensation in the two-cylinder as compared with the three-cylinder engine would compare fairly with the differences of consumption.

Mr. A. C. KIRK. My lord and gentlemen, this paper that I have had the honor of reading I am glad to say has been subjected to a very full and very fair criticism. Mr. Longridge was the first to criticise the paper, and I will say generally I think he took the ground that expansion of steam was expansion of steam, do it as you like, do it in one cylinder or do it in a dozen cylinders. So it ought to be, but in this world things are not quite as we think they ought to be; and, though no doubt Mr. Longridge has a good many interesting experiments to refer to, the whole mercantile marine of this country has been one big experiment on this subject for the last twelve years, and the result is that it has been proved that expansion in two cylinders has worked more economically than expansion in one. I will not go further than simply to adduce that fact, which conclusively settles the question. He then refers to superheating as giving a greater economy. Of course it does, because by it you get the high temperature, and that is what you want. If we could get the temperature of steam without so rapid an increase of pressure, we should be very glad, but unfortunately the pressure keeps rising very much faster than the temperature. Now he seemed to think that the difficulty was in making a superheater. There is no difficulty whatever in making a superheater; I have superheated steam so that it would set a piece of wood on fire when it was blown on; but the difficulty is to produce an engine to work with highly superheated steam. Although I am prepared to take in hand an experiment about as freely

as most people, I am certainly not prepared to advise any of my friends, as a commercial matter, to put money in work with which I have to do, of so purely experimental a type, and which may be just as likely to fail as not, and perhaps more likely. We know that with highly superheated steam you cannot lubricate the surfaces, and that they wear excessively. The only approach to lubrication that is practicable is by keeping the cylinders as cold as possible, and then you can condense a little steam on their inner surfaces, but then the goodness of the superheating has to a great extent been lost. Now, when I come to Mr. Cowper's remarks I feel some diffidence. Mr. Cowper is an old authority on this subject, and to Mr. Cowper is largely due, at all events practically due, the introduction of the present receiver type of compound engine. Therefore, as I say, I speak with some diffidence. When Mr. Cowper takes the indicator diagram there, and the whole expansion is supposed to be properly performed, no doubt he is quite right when he says 12 per cent. or so is about all you can gain. But while this is true, it is largely modified in practice by the way in which it is carried out, and this is the point which formed the subject of the paper. Steam jackets were for a long time considered to be the panacea of all evils. Certainly steam jackets do this much: if the engines go slow enough, and give the heat time to be conducted from the outside of the barrel to the inside of it, the steam jacket will keep the inside of the cylinder at the temperature of the admitted steam; there will be no steam condensed, and if no water comes into the cylinder the only steam evaporated towards the end of the stroke will be the steam condensed in doing work. However, I will come to that point by and by. But then, it is not always the case that we get steam free of water. My friend Mr. Macfarlane Gray says that the best steam we have is water suspended in perfect gas, but we even get a little beyond that, and get in ordinary steam a good deal of actual unquestionable water suspended in it, and when we get to that length the effect of the steam jacket is to make the cylinder a steam boiler. Now there is not much harm that happens in a high-pressure cylinder by this evaporation of water, but when you get down towards the end of the expansion of the steam, and you begin there to evaporate any tangible quantity of water, I cannot at all see what good you have got by it. You certainly would better have got that water straight out of your cylinder than have gone and spent that heat direct from your boiler, the heat of the high-pressure steam, to generate a certain quantity of very low-pressure steam that has only a very little work left for it to do. I cannot see that when the jacket begins to have that function it is doing right; and when you get to long ranges of expansion it is hard to see how the jackets can help doing it. There is another method, besides the use of jackets, of preventing the large variations of temperature (in which there is no harm in itself if the cylinder temperature could follow the temperature of the steam), and along with that the large condensation and re-evaporation that uso take place, and that

is to divide the expansion into steps, and to keep the change of temperature in any one cylinder comparatively small. You might conceive the steps so numerous and the range of temperature in each so small that jackets would be quite useless. I think in practice, in the ordinary marine engine, we are now in this position, that jackets are not of very much use. I do not think it is very necessary to dispense with them; they are convenient, at all events, to heat up the engine, and though some little economy can be gained at sea by them, if they are carefully worked, at the best it is not much; and we know that, as Mr. Macfarlane Gray suggested, steam jackets are about the last thing marine engineers bother themselves about. It is just as likely that you will find the steam jackets filled with water to the top as anything else, or, if not, on the other hand, the drain cock is wide open, blowing away a continual rush of steam into the hot well, and probably overboard.

Mr. Cowper. Not necessarily.

Mr. A. C. Kirk. Hence I really do think the receiver type of engine (apart from the Woolf engine) got a great start from not depending materially on the efficient use of steam jackets, whereas the Woolf engine did. In Mr. Elder's Woolf engine (for such his compound engine was), for instance, in the four-cylinder engines of his paddle boats, the steam in the high-pressure cylinder, after working through the range of expansion to the end of the high-pressure stroke, expanded simultaneously in the high and low pressure cylinders, and the high-pressure cylinder was reduced to the temperature of the release of the steam to the condenser. Thus the high-pressure cylinder passed through the full range of temperature possible, excepting the drop from the temperature of release to that of the condenser, and the practical effect in these paddle engines, if the jackets were neglected in the very least degree, was to produce such a cracking noise from water, in the bottom of the cylinder particularly, as would rather frighten you. I confess at first I did not see the cause with the first pair I had through my hands, and got a bit of a scare, but one of the old hands said, "Let me open the jacket drain cock at the bottom." He did so, and it was all right. What I have said comes to this: that I do not quite sympathize with what Mr. Cowper has colloquially called his "hot pot." I confess if he could heat it with waste heat, good; but if we take the heat out of the boiler to heat it, I do not quite see the advantage of it.

Mr. Cowper. Might I add one word of explanation? With isolated steam 480 degrees doubles the bulk, but it takes 1,000 degrees to make a bulk of steam from water; therefore you get it at something less than half the cost. Isolated steam heated produces more power than water heated.

Mr. Kirk. Mr. Cowper spoke of the Briton burning 1.3 lbs. of coal per horse-power, but I do not quite understand it, because I do not think it has ever been done since in the navy, and our friends in the navy are always very sharp in adopting the best practice. Perhaps it can be explained further.

Mr. Cowper. It was an Admiralty trial under Government; it was 1.98 on the measured mile, and 1.6 and 1.4 at various speeds, and 1.3 at 10 knots.

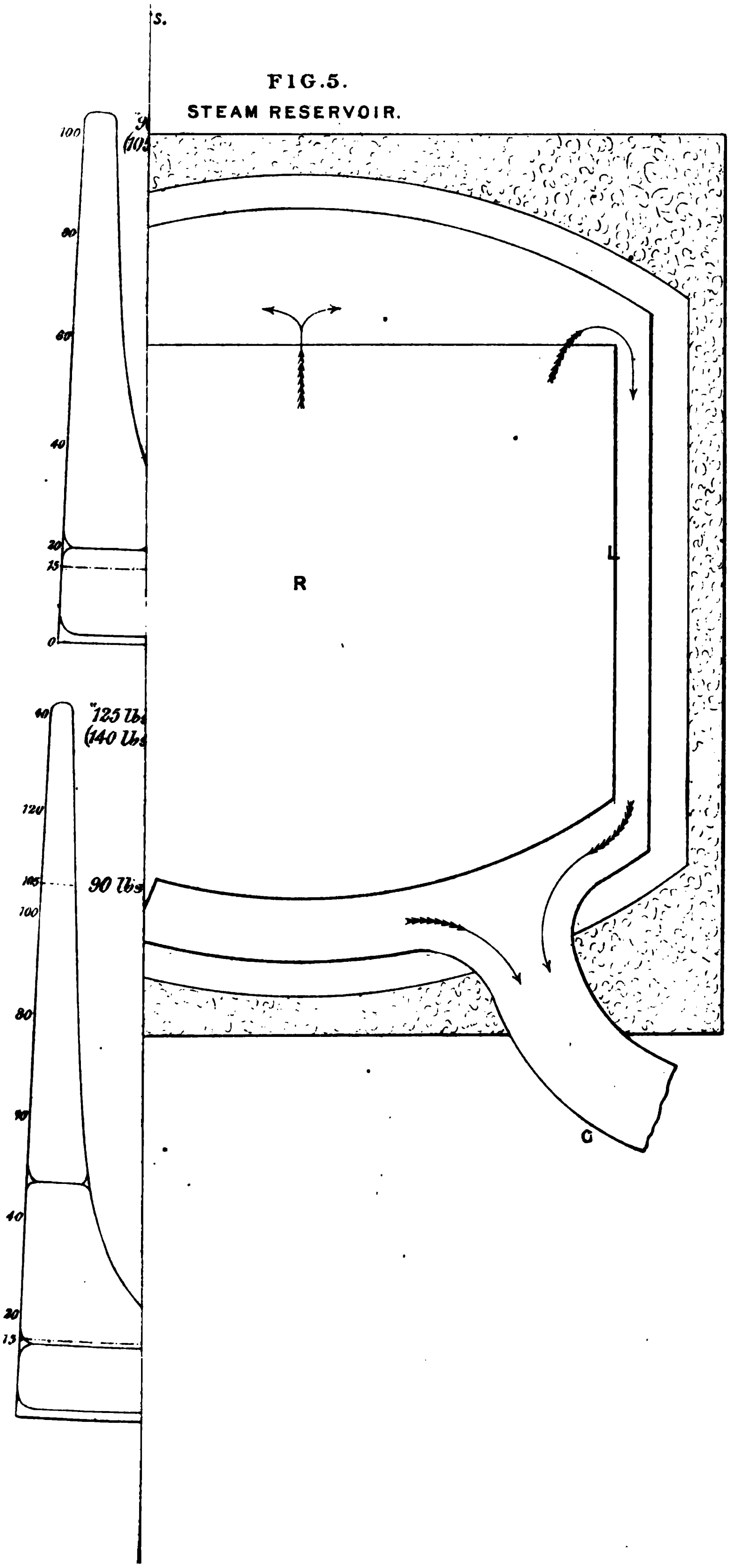
Mr. A. C. Kirk. I do not think it is necessary for me to refer to Mr. Macfarlane Gray's remarks. We shall have his paper, no doubt, and I dare say more fully developed. I can only thank you for the patience you have had.

Mr. W. Parker. At this late hour, my lord, I will content myself with only a few remarks. I listened with great interest to what fell from Mr. Longridge, whose opinion, arrived at in the full light of his recent researches into the preformance of land engines, is of much value; but it is contrary to the experience of all marine engineers. I should like to draw attention to the fact that measuring the efficiency of a land engine is one thing, and measuring the efficiency of a marine engine quite another thing. In a land engine you can accurately ascertain the work performed, and the amount of water used in generating the steam so that the efficiency can be easily determined, while in a marine engine the conditions are very different. In the first place, marine engines are so powerful, as compared with those on land, that it would require a very large and complicated appliance to measure the amount of feed water used; and, secondly, even if this were made, it would be almost, if not quite, impossible to maintain precisely similiar conditions as regards weather, &c., in different trials, so that we can never hope to have such accurate data for marine engines as that obtained from land engines by Mr. Longridge. Mr. Longridge's opinion that we should expand our steam in one cylinder only was shared by many of our eminent marine engineers years ago, and I may give you one instance out of several where the idea had a fair trial and failed. Many of you will doubtless remember that in the year 1872 the Allan Line went to the expense of fitting their steamer Circassian with a pair of engines of about 2,500 i. h. p. to work at a pressure of 60 lbs. per square inch, having an early cut-off, and expanding the steam in one cylinder only. These engines worked for a few voyages in the Atlantic trade, and it was found that the shock at the commencement of the stroke and the consequent vibration of the vessel were so great as to render it impossible, if not even dangerous, to drive the engines at full speed, while the consumption was found to be greater than with the engines of the same power expanding by two cylinders. So far convinced was Mr. Wallace, the company's superintending engineer, of the unsuitableness of these engines that he advised the company to replace them with ordinary compound engines, which were accordingly fitted. This is only one of several cases where it has been proved that expanding steam in a single cylinder in a marine engine is not the most economical arrangement for the propulsion of a steamship. With regard to Mr. Cowper's remarks about jacketing, I may say that our experience in the Aberdeen amounted to this: Both the middle and low pressure

cylinders are jacketed with steam from the boilers, and the engines were worked for three or four hours with the steam in the jackets, the drain water being gathered in two tanks, so as to enable us to ascertain the weight of steam used in the jackets, and indicator diagrams being taken at intervals of about a quarter of an hour; the engines were then worked for a few hours with the jackets free from steam, indicator cards being again taken at short intervals; and on comparing the results we found that if about the same weight of steam that was used in the jackets were utilized in the cylinders we should get practically the same result. Mr. Scott asked about the curves exhibited on Fig. 2 (Plate II, Mr. Parker's paper). Well, the indicator diagrams shown on that figure are taken from a two-cylinder compound engine, constructed to work at a pressure of 125 lbs. per square inch—the same pressure as is used in the three-cylinder or triple expansive engine shown on Fig. 1 (Plate I, Mr. Parker's paper); and it will be observed that on the h. p. card there is a considerable space between the curve shown by the diagram and the adiabatic curve, or curve of perfect engine, the cause being undoubtedly the large amount of initial condensation taking place in that cylinder. Mr. Joy made a remark about the steamer Howard, built by the Barrow Shipbuilding Company. She had an engine very similar to that represented in Fig. 2, and her engines worked at the same pressure; but the ratio of cylinders was very different in the two cases, very little expansion taking place in the h. p. cylinder of the Howard. In this paper I have carefully avoided saying anything about consumption of fuel in relation to indicated horse-power, and have merely tried to show the loss that must take place in an engine working at high pressures and with long ranges of temperature from condensation only; but the actual efficiency in any one engine depends very largely upon the details of the slides and ports, so that in considering the efficiency of any type of engine, it is not sufficient to draw comparisons between it and an engine of different type, unless the arrangements alluded to are equally well designed in each case.

The PRESIDENT. It only remains for me to convey our thanks to Mr. Parker and Mr. Kirk for their most interesting papers.

FIG.5.
STEAM RESERVOIR.



III.

LLOYD'S RULES AS AFFECTING MARINE BOILER CONSTRUCTION.

By J. T. MILTON, Esq., *Assistant Chief Engineer Surveyor to Lloyd's Register, Member.*

[Read at the twenty-third session of the Institution of Naval Architects, 30th March, 1882; the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

At a recent meeting of the Institution of Mechanical Engineers at Newcastle, a notable paper was read by Mr. C. F. Marshall on "The Progress of Marine Engineering," in which appears a statement to the effect that "the ordinary marine boiler, incumbered as it is by the regulations of the Board of Trade and of Lloyd's Committee, does not admit of much reduction in the weight of material or of water carried when working." In the discussion which followed, Mr. Kirk remarked that "a locomotive boiler which is considered safe on a railway would not be passed for use in a steamer at the same pressure"; while Mr. Marshall explained that though he did not complain of the restrictions, "yet restrictions existed, and as long as they did exist they would have difficulty in modifying the form of marine boilers materially," and he indorsed the remarks as to the locomotive boiler on shipboard.

Such statements as these, coming from such well-known authorities, and so widely circulated as they have been, will, in my opinion, tend to produce the erroneous impression in the minds of ship-owners that Lloyd's Register is an obstructive institution, preventing them from obtaining all the advantages of high-steam pressure. In the present paper I shall endeavor to prove that this is not the case, by showing that the only restrictions imposed by Lloyd's Register are such as are actually demanded by considerations of safety.

I would here venture to remark that the experience of the Surveyor's to Lloyd's Register, gained in the periodical examinations of marine boilers of all types, gives them exceptionally good opportunities of judging not only of the original strengths required for the structures to withstand the wear and tear to which they are subjected, but also of the defects of different modes of construction; as probably no persons in the kingdom have so many and such varied examples of marine boilers of all descriptions, of all ages, and in all stages of corrosion passing continually under their inspection.

First, as to the restrictions regarding forms or types of boilers. Lloyd's Rules require any novelty in the construction of either boilers or machinery to be specially submitted to the committee at as early a

date as possible. The merits and demerits of each plan thus submitted are very carefully considered, and in very few cases indeed have the committee of Lloyd's Register refused entirely to sanction any arrangement which has been proposed to be actually used. In every case of novel boilers in which it was evident that the original design or a modification of it fully provided for safety, unconditional approval has been given, whilst in many cases in which it is considered that the continued efficiency and safety of the boilers for long periods are dependent upon more than ordinary care and attention being bestowed upon them, they have been approved of, subject to their being frequently submitted to survey. I may mention that nine types of novel boilers have been passed unconditionally by Lloyd's Register, six types have been approved of subject to frequent resurvey, while the Perkins and Howard boilers are the only types which have been strongly objected to and disallowed.

Of these novel boilers, those of the last-mentioned types have already been taken out of the steamships Wanderer and Howard, respectively, at the instance of their owners, and replaced by others of ordinary form; of the nine passed unconditionally, two have been taken out of the vessels in which they were fitted, and two have been materially modified, after a few months' trial, on account of their not giving economical results equal to the anticipations of their designers, not in either case on account of their having proved to be dangerous; of the five types of boiler approved of subject to frequent examinations, the results in every case have proved that the restrictions prescribed have been absolutely required, one type of boiler fitted in two coasting steamers having been found to have become somewhat damaged in the short space of three months between consecutive surveys; whilst two sets of similar boilers fitted in vessels not classed in Lloyd's Register making longer voyages, and therefore not subjected to the same amount of supervision, became absolutely dangerous, through their having been allowed to become dirty, and these have had to be removed.

I think that this clearly shows that so far as *form* of boilers is concerned, any restrictions imposed by Lloyd's Register have not been uncalled for, while I think it will be conceded without question that on the subject of the use of mild steel for marine boilers coming to the front, the action taken by Lloyd's Register in approving of the material on being satisfied of its quality, greatly facilitated its introduction.

Next, with regard to the restrictions imposed by Lloyd's Register in the matter of strength. All structures must be made with a strength in excess of the load which they are designed to bear, in order to provide for their safety in the event of the occurrence of any or all of the following contingencies:—

Latent defects or flaws in the material.

Errors or defects of workmanship.

Exceptionally severe strains which may possibly be brought to bear upon the structures by causes other than those which they are designed to resist.

The weakening effects of corrosion or wear and tear.

Upon the relative probabilities of the occurrence of these contingencies in different structures depends the excess of strength required in their design, the proper allowance for each of the causes being determined by actual experience. In the case of marine boilers, it is with the last-mentioned cause that we have principally to deal. As I have previously stated, the surveyors of Lloyd's Register possess exceptional advantages in observing the effects of corrosion, and this experience was brought to bear upon the subject at the time when the rules for the strengths of boilers were revised some three years ago.

I have recently examined into the original scantlings of a large number of boilers made by eminent firms of marine engineers, and fitted on board vessels classed in Lloyd's Register just prior to the time when the officers of that society commenced to survey machinery, confining my examination to the cases in which the boilers had not been built under Board of Trade supervision, and where the manufacturers were, therefore, to a large extent untrammeled and left free to carry out their own ideas on the subject of strength; and in every one of these cases the boilers would now be allowed by Lloyd's Rules to work at a pressure considerably above that for which they were designed. I find that on an average they would be allowed 25 per cent. greater pressure as regards the cylindrical shells, 44 per cent. greater pressure as regards the combustion chamber plating, 38 per cent. as regards stays, and 38 per cent. as regards furnaces, while in many cases the strengths far exceeded these averages. Of course, in regard to the surplus strength of furnaces, and to some extent also in some cases in regard to the combustion chamber plates, it is difficult to work exactly down to any fixed rule, as an extra $\frac{1}{32}$ on the thickness of the plate will increase the strength about 15 per cent.; but there is no such difficulty in regard to the shells, and this large margin shows that Lloyd's Rules in this case have not thrown obstructions on the previous practice, while the fact that the present rules are now worked down to by nearly all makers seems to show that, on the contrary, by inspiring confidence in less scantlings, they have really led the way for an increased pressure being carried by the same type of boiler to the extent of at least 25 per cent. That this confidence has not been misplaced is shown by the remarkable immunity from serious accidents of boilers which have been passed by Lloyd's Register; but, at the same time, I will endeavor to show that much, if any, further reduction of scantling would be imprudent, taking into account the deterioration to which all parts of boilers are liable.

First, let us take the case of cylindrical shells, the great thickness of which is the greatest obstacle at present to the carrying of increased pressures with the present type of boiler.

Lloyd's Rules provide a different coefficient of stress for boiler shell plates, according as the longitudinal seams are made with lap or butt-strapped joints, and as the rivet holes in these joints are drilled or

punched, the coefficients also varying with the thickness of the plate employed. Messrs. Parker and John some time ago carried out a series of experiments in order to determine the loss of strength of steel plates occasioned by punching the rivet holes without subsequently annealing the plate, and at the same time, in order to obtain comparative results in the case of iron, they made similar experiments with various thicknesses of iron plates. Their experiments showed, without doubt, that iron loses considerably in strength by punching, although to a less extent than steel; but we can scarcely assume that the loss of strength is less than 10 per cent., as in most cases it was found to be more than this. Lloyd's Rules provide for a stress of about 10 per cent. less being borne by punched plate than by the same plate in which the holes are drilled; at the same time, as experiments do not show that rivets are any stronger in drilled than in punched holes, the same stress is practically allowed upon them in each case by reducing the larger coefficient of stress by 10 per cent. so far as the rivets are concerned for cases in which the holes are drilled. Practically, if boiler plate unpunched is credited with a strength of 20 tons per square inch, Lloyd's Rules credit the punched plates, and also the rivets either in punched or drilled holes, with a strength of 18 tons per square inch.

Lloyd's Rules in new boilers made with lap joints allow a stress of 7,750 and 8,500 pounds per square inch with thin plates, say $\frac{6}{16}$ thick, according as the holes are punched or drilled, in $\frac{9}{16}$ plates they allow 8,250 and 9,000 pounds, in $\frac{13}{16}$ and thicker plates they allow 8,500 and 9,500 pounds per square inch, respectively. These figures, neglecting all possible imperfections of workmanship, and assuming a strength of plate of 18 and 20 tons per square inch, respectively, give a margin of strength of 5.2, 4.9, and 4.7 times, respectively. If these plates become uniformly corroded to the extent of $\frac{1}{16}$ of an inch, the same working pressure will produce stresses of about 9,300 pounds and 10,200 pounds per square inch in each case, the margin of strength being in all cases reduced to about 4.4 times, while a corrosion of $\frac{1}{8}$ of an inch in the three cases reduces this margin to 3.5, 3.8, and 4.0, and a corrosion of $\frac{3}{16}$ brings these numbers to 2.6, 3.3, and 3.6, respectively. This amount of corrosion is not an uncommon amount to be met with in boiler shells six or eight years old, and would so lessen the strength of the thinner plate as to demand a considerable reduction of the working pressure, while even in the thicker plate the margin of strength is less than many prudent engineers would care about working with, considering that these figures do not provide for any contingency as to defective material or workmanship.

In shell plates fitted with double butt straps, if the straps are originally made somewhat thicker than the half of the thickness of the plates, a considerable uniform corrosion can take place without weakening the joint at all. For instance, suppose the butt straps are made $\frac{5}{8}$ the thickness of the plate, and the strength of the joint is made to be originally

75 per cent. of that of the solid plate, then a uniform corrosion to the extent of $\frac{1}{4}$ of the thickness of the plate will reduce throughout to that of the riveted joint, so that the only loss of strength the structure would receive is that gained by the crossing of the joints—an amount which experiments have shown to be very small with the wide plates now generally used. On the other hand, corrosion is often very far from uniform, and in the event of a leaky joint—a by no means unknown occurrence in the bottoms of boilers—the corrosion always found at neglected leaky places will attack the weakest place, the joint itself, and will really weaken the structure. Taking these points into consideration, Lloyd's Rules allow a greater stress with double butt strapped joints than with lap joints, although not so much the greater as the uniform corrosion theory alone would justify.

Coming now to stays: The greatest stress allowed is 6,000 pounds per square inch calculated from the smallest part of the stay, both with large and small stays; but when we consider that large stays are almost invariably made with raised threads and have the ends welded on, and that the strength of the welds cannot be relied upon to be equal to that of the rest of the bar, and also that the smaller screw stays have the threads left on them, and are thus to some extent protected from corrosion, we see that a very appreciable amount of corrosion on each will be required to bring their strength to an equality.

In flat plates supported by stays, and also in circular furnaces subjected to external pressure, we find a deal of difficulty in estimating the pressure which will produce a given amount of stress. In all the experiments on these joints with the results of which I am acquainted, the principal point observed has been the ultimate pressure producing rupture or collapse. In some only of the cases have the bulges and sets at the different pressures been carefully observed, while the critical pressure at which permanent set was first produced has in most, if not all, of the experiments on flat plates, to be estimated to be between two pressures, more or less widely apart, as the stages at which the observations were made were few or numerous. For instance, in some of a very extensive set of experiments made recently, it is recorded that there was no set or bulge at 50 pounds, and $\frac{1}{64}$ set at 100 pounds in one case, while in another there is no set recorded at 200 pounds and $\frac{1}{32}$ set at 300 pounds pressure, the results thus giving no definite knowledge as to the exact proof pressure within 50 per cent. of the truth in the first case and 33 per cent. in the second. Experiments conducted in this way are nearly useless, so far as concerns any light they throw upon the elastic or useful strength of flat plates supported by stays.

Now in most structures the stress produced in any part is proportional to the load producing it, not only up to the elastic or proof stress, but also up to the ultimate strength of the material, so that an experiment giving the ultimate strength of the structure will also give a fair idea of its proof or elastic strength, if the ratio of the proof stress

to the ultimate strength of the material is known. In the cases both of flat plates and furnaces, however, this does not hold good. With both, so soon as the proof strength is exceeded, the ratio of stress to load becomes widely different from what it was before. In the case of the flat plates, when a large amount of bulging has been produced by the pressure, the material is evidently wholly in a state of tension, whereas up to the proof strength the material must be in tension only where it becomes convex, and in compression where it becomes concave. The ultimate pressures such plates will bear are therefore, with ductile material, considerably more than double their proof pressures, being more proportional to the ductility or power of stretching than to the original stiffness or rigidity of the plates. Further, it is to be remarked that in all boilers it is impossible to keep the plates exposed to the action of the heat of the furnace gases absolutely clean, and a very thin scale upon the water side of the plate is sufficient to offer such an obstruction to the flow of heat through it that the plate must become somewhat overheated, and when more than a very thin scale is allowed to accumulate the plates must get very hot and lose considerably in rigidity. This contingency has to be provided for, as well as the loss of strength due to corrosion. As a matter of fact, we find that very few, if any, old boilers have their chamber-plates unbuckled, whilst, in very many comparatively new boilers, the plates are largely bulged through overheating alone, although they are found to be of their original thickness and to have a large margin of strength when cold. These considerations must not be lost sight of when comparing the pressures which are allowed in practice with the ultimate strengths found by experiments upon cold plates.

Lloyd's Rules give a larger coefficient for $\frac{1}{2}$ inch than for $\frac{7}{16}$ plates, thus to some extent providing for corrosion, and they also give a greater coefficient for plates in which the stays are fitted with nuts than for those in which the heads are riveted, on account of the greater security afforded by the former method of securing the stays where buckling of the plates is to be apprehended. With regard to the rules as they are at present for flat plates, I think I may safely say that they do not throw any serious obstacle in the way of working with a pressure of 200 pounds per square inch, which largely exceeds any pressure that is either being used or contemplated at present, whilst experience gained with old boilers does not warrant any reduction of scantlings from those now required.

In the cases of furnaces, we very much require some elaborate and costly experiments to determine the true law as to their actual strengths, the number of experiments made on full-sized furnaces up to the present being very small. Fairbairn's experiments were made principally on small models, and the rule he deduced, or modifications of it, have generally been used by engineers in designing flues; but, unfortunately, this rule does not well apply to the dimensions of furnaces usually

adopted in marine boilers. For instance, it cannot be considered that a flue 7 feet long and 3 feet 6 inches diameter, made of ½-inch plates, would have its strength doubled by fitting an anti-collapsing ring at the middle of its length—a result which Fairbairn's Rule would lead one to suppose.

Recently, Mr. Fox, of Armley, Leeds, has made some experiments on flues about 7 feet long and 3 feet diameter, but as his experiments were made solely as a means of comparison of plain *v.* corrugated flues, various lengths and diameters were not necessary for his purpose, and were accordingly not used. In these experiments it is to be noted that all the plain flues collapsed at a much lower pressure than that deducible from Fairbairn's Rule, from their dimensions and thickness.

Although there is such a lack of experiments giving the actual collapsing pressures of furnaces, we have an immense number of experiments of a negative character. The great number of actual furnaces at present running safely show that their collapsing pressure is at least greater than that at which they work, and thus prove that the rules by which they were designed provide at least sufficient strength for practical purposes, while not one of the many cases coming under my knowledge of actual collapse of furnaces at the ordinary working pressure has been due to constructive weakness. In some of the cases the cause has been clearly seen to be shortness of water, in others it has been the accumulation of lime scale, in several others it has been the accumulation of a peculiarly tenacious scale, apparently principally composed of precipitated carbonate or sulphate of lime from the water, intimately mixed with mineral oil from the cylinders. A very thin coating of this greasy deposit appears to be quite as non-conducting as a much thicker deposit of crystalline scale. Several collapses are traceable to a deposit of common salt, through allowing the density of the water in the boiler to gradually accumulate up to the saturation point; and in one at least of the two or three cases in which the furnaces have lately collapsed, but have been found to be perfectly clean on the vessel's arrival in port, this accumulation of salt has been the cause of the accident, but the engineers have, by freshening the water after the accident, dissolved the deposit. All these cases come under the category of overheating, not of structural weakness, for although the use to which furnaces are put necessitates their being raised to a temperature somewhat above that of the steam, and therefore requires their being made thicker than would suffice to withstand the working pressure if they were always cold, yet no thickness of plate whatever will give sufficient rigidity at a red heat to prevent such accidents if they are subjected to such conditions.

In investigating the strength of furnaces, and in deciding upon what margin of strength is necessary to provide for safety, it must be remembered that, unlike most other structures, their proof strength is also their ultimate strength. Up to their proof strength their material is

subjected to simple compression, but so soon as this is reached deformation commences, and the material then fails, by cross bending, at a much lower pressure than it stood before alteration of form. In fact, a slight deformation produces such a considerable loss of strength in a tube subjected to external pressure, that it appears that the important factor to be ascertained in any flue of given diameter and thickness is not so much its length as its roundness.

Lloyd's Rules for furnaces are a modification of Fairbairn's Rule, with a limit as to the effect of short lengths. They do not take into account the form of the joint of the plates, whether welded, lapped, or butt-strapped, or whether the rivet holes are punched or drilled, as careful measurements of a great number of furnaces, as actually made, show that the roundness, or rather the want of roundness, in practice does not depend upon the joint at all, but depends upon the care bestowed on the workmanship, the furnaces most nearly round being often found to be those with lap joints, while welded flues are often far from cylindrical.

If further experiments are made upon the strength of flues, I do not think that they can very much affect the existing pressures carried by furnaces of the proportions at present used, but if the rules for furnaces do bear hardly upon engineers, I feel sure that if they will prove by actual experiment, as Mr. Fox did with his corrugated flues, that any modification of them will provide for safety, making due allowances for the weakening by corrosion and other contingencies, the Committee of Lloyd's Register would not be found to raise obstructive objections.

With regard to the statements I quoted at the commencement of this paper as to the use of locomotive boilers for marine purposes, I would wish to remark that the conditions under which marine and locomotive boilers work are widely different. Locomotive boilers may be kept under constant skilled supervision, while marine boilers are often away on voyages of many months' duration. Locomotives, again, not being fed with sea water, do not in general suffer so much from corrosion, nor from the accumulation of non-conducting scale, as marine boilers, and therefore they do not require at first such a large margin of strength beyond the working pressure; but I think it very doubtful if *old* locomotive boilers are allowed to run with less margin of strength than *old* marine boilers before they have their pressure reduced or are broken up, and it is at this stage of their existence, if at all, that their relative strengths should be compared.

At the present time, while many engineers are looking to the locomotive type of boiler as offering some advantages in point of weight over the marine boiler as at present in use, it may be interesting to know that one of the boilers passed unconditionally by Lloyd's Register was of the locomotive type, but its performance during the first voy age of the vessel was so unsatisfactory that the owner ordered another boiler of ordinary type to replace it, and it was taken out of the vessel

after about six months' work. In this boiler, even after three months' work, the sides of the fire-boxes commenced to bulge between the stays, although they were kept as clean as it is possible to keep marine boilers, and the water spaces were thought to be ample, being 6 inches wide at the lowest part, the circulation of water in these spaces also being supplemented by external circulating pipes. The flat-sided fire-box does not seem, from this, to be well suited for marine boilers, and further experience with this and with other novel boilers has shown that when the fire-door is open, the cold air rushing in direct to the tubes contracts them at the ends and causes them to leak; whereas in the ordinary boiler this air has to pass close over the fire before reaching the tubes, and consequently gets heated and does not do so much mischief.

Up to the present time, I believe that the reason why the ordinary type of boiler is almost invariably applied for marine purposes is not on account of any restrictions imposed upon new types by Lloyd's Register or by any other controlling body, but solely because the great difficulties of designing a marine boiler which shall meet all practical requirements better than those of ordinary type have been greater than the inventive genius of our marine engineers has been able to overcome. In conclusion, let me say that I feel sure if marine engineers will design and submit any improved boiler to the Committee of Lloyd's Register it will have very careful consideration, with a view to encouraging any *safe* advance, but *safety* must be the first consideration.

DISCUSSION.

Mr. JOHN SCOTT. My lord, may I be permitted to make an observation or two on this paper? I certainly wish to congratulate Mr. Milton upon the very fair way in which he has gone over all the points involving difficulties in construction of marine boilers, explaining so fully to the marine-engineering profession what his society is prepared to do. It is impossible in the time allotted to go over the whole of the points involved in this paper; but if I may be permitted, I should like to make a few observations on those paragraphs of Mr. Milton's paper which deal with the question of furnaces, both because he seems to give considerable prominence to that part of the question in his paper, and because certainly in my opinion, and I may say in that of my brother marine engineers, that is possibly the most difficult point connected with the construction of marine boilers, if we are to proceed at the advanced pressure which the paper we heard yesterday by Mr. Kirk, and the diagrams shown on the wall in illustration of Mr. Parker's paper, lead us to imagine is likely to take place in the future. Mr. Milton refers in the case of furnaces to some recent experiments which have been made to determine whether the law which has been accepted, as he says,

and I believe with truth, by most marine engineers, namely, that put forward by Fairbairn many years ago as to the strength of circular furnaces, was true or not. I had the pleasure of conducting those experiments in conjunction with Mr. Fox, whose name is mentioned in this paper, happening to be chairman of the company in which he is managing director; and I believe the experiments which were then conducted were the first and only ones which have ever been carried out on a large scale with the view of determining what the strength of circular furnaces ought to be. Those experiments were necessarily, in view of the great expense attending them, restricted to furnaces of one length only; and I certainly agree with the opinion put forward by Mr. Milton in his paper, that if anybody could be found to undertake the expense, it would be very desirable if that series of experiments were extended and the whole truth of the matter were ascertained. I think it certainly would be a very proper matter if the Board of Trade would undertake this question. I must on this occasion acknowledge the very great interest which the Board of Trade engineer officers have taken in following the course of those experiments, and the very great assistance also which they have given, equally with the officers of Lloyd's Registry, in attending those experiments; but I think it would be exceedingly desirable if the suggestion made by Mr. Milton were carried out by somebody, and certainly the Board of Trade, as representing the country, would be the most natural authority by which it might be carried out with advantage. I would only wish to add one or two other observations, and one of them is in reference to the paragraph in Mr. Milton's paper, referring to the desirability of having the furnaces as circular as possible. He states that " a slight deformation produces such a considerable loss of strength in a tube subjected to external pressure that it appears that the important factor to be ascertained in any flue of given diameter and thickness is not so much its length as its roundness." Now, from having followed a number of those experiments, I can bear out very fully what Mr. Milton states. It was a matter of extreme surprise to all those present, when the first ordinary circular furnace was collapsed by the Leeds Forge Company, to observe that after the first change from the circular to a slight depression of curve how very quickly the furnaces came down. I am sure that anybody who has not seen a furnace proved to destruction would be astonished by the facts that have been brought out by those experiments. It must certainly be the case that an enormous number of the ordinary furnaces which are now in use, not only in the steamers of this country, but also in the land boilers of this country, are on the very point of collapsing, though the persons in charge of them do not know that it is so. Seeing that that is the case, clearly proved by the Leeds experiments—and I know Mr. Parker, who has seen a great deal of those experiments, will bear out what I say—it is of enormous importance for every user of a marine furnace to ascertain that he is getting it as circular as it is

possible to make it. But while I state those facts to confirm what Mr. Milton has placed before us, I would wish to state that this is only true when the furnace is a plain circular tube. It ceases to be a fact when the furnace is divided into short lengths, such as those produced by Mr. Fox's corrugated furnace. That fact has been brought out most undoubtedly under the very extreme pressures to which those furnaces have been subjected, that collapse does not take place with the rapidity it does in a common furnace, simply because the circular projections in the furnace constitute a source of resistance which prevents the tendency to collapse which takes place in a plain round furnace. And I certainly would wish to take exception, as regards all forms of the furnaces, to the statement made by Mr. Milton in his paper. There is also another point which has been brought out in the experiments to which I have referred, which is this: that the ordinary form of supporting a furnace by adding a joint in the center, such as that known as Adamson's joint, or by the addition of rings, does not add the strength which the use of Fairbairn's formula would have led one to anticipate. Certain experiments made by the Board of Trade by the machinery prepared at the Leeds Forge has led conclusively to that result, though those experiments were not made at the time exactly with the view of determining the question. I think all those points go very strongly to make it most desirable that a series of elaborate experiments should be carried out, with a view of putting this most important matter clearly before the marine engineers of the profession.

Mr. Ravenhill. I shall be pleased to offer a few remarks, because it is now some years since I first made the acquaintance of Mr. Parker, at the time when he was an official of the Board of Trade. That acquaintance was caused because a question arose as to the strength of certain boilers that we had fitted on board some vessels at Newcastle, basing our experience on that obtained during the Crimean war, when we fitted many high-pressure boilers in vessels of the Royal Navy. At that time 60 pounds of steam—in those particular boilers it was 75—was considered as a very heavy pressure. Mr. Parker carried out (and he was quite right) the instructions he received from headquarters, and he told us he could not pass those boilers for 75 pounds' steam. I went to the Board of Trade, and I said, "Gentlemen, those boilers have stood an hydraulic test of 150 pounds on the square inch; that is considered a sufficient proof by the Admirality that those boilers would be competent to do their work." After very considerable trouble we succeeded in obtaining a very short-lived certificate for that vessel from the Board of Trade. But, before that certificate had expired, the Board of Trade were in London brought face to face with one of the greatest marine engineers, if not the very greatest marine engineer, that England ever produced. I allude to the late Mr. John Penn. He had fitted for a well-known London firm boilers to carry 70 pounds; and when he went to the Board of Trade, or rather when the owners went there for a certifi-

cate, 50 pounds is all the Board of Trade would allow them to carry. The Board of Trade, as in our case, ignored the hydraulic test: they would not listen to it. The owners of that vessel treated the Board of Trade in a different way to the owners I myself had to deal with at Newcastle; the owners simply said to the Board of Trade, in effect, "We have more confidence in Mr. Penn's opinion than we have in yours, and that ship goes to sea, and we defy you to stop her." Now, the boilers of that vessel did well, and did good service, and I have since had the gratification of hearing from Mr. Parker himself that the lives (if I may use the term) of the boilers above alluded to have caused him to reflect seriously over the question of the strength of marine boilers; and we know now that some little time back Lloyd's Rules for shells of boilers were reduced in scantling; and I know Mr. Parker will bear me out, and remember the fact that he said, "It was those very things which caused me to reflect what could be done, and I am now doing it, principally based on that Admiralty experience." The pressure of course at that time was not all we had to contend with in the management of boilers. It was a great jump, and we not only had to make the boilers, but we had to make the men to manage those boilers, because the management at sea of boilers working at those pressures is totally different to that where boilers are working, or were working, at 25 or 30 pounds steam, and to which the engineers of those days had been accustomed. Boilers that were then questioned, although they had a certificate given to them by the Board of Trade, in a steamship belonging to a well-known London firm whilst I had the supervision of them, ran over 112,000 miles. They were in the year 1874–'75 subjected to such repairs as might have been anticipated after running three or four years, and the last time I heard of those boilers they were still on board the ship, and I believe they are there now. Since those days we have seen a set of rules prepared by the Board of Trade giving certain coefficients for strength, and in giving those coefficients, they, for the first time, acknowledged the use of superior metal or iron in high-pressure boilers. The boilers that I have alluded to were made of Yorkshire iron and "best best" Staffordshire iron, and probably, at that time, as good iron as could possibly be procured. We have seen the hydraulic test, which we advocated and which was ignored, now become a standing rule, and we have seen pressures grow and grow; we have seen Mr. Parker become the chief engineer of Lloyd's Registry of Shipping, and I say it without fear of contradiction, that steamship owners, in fact, the whole commercial marine, owe a very deep debt of gratitude to Mr. Parker for the views that he has brought to bear, and has been carrying out since he has been at Lloyd's; for I believe that if it had not been for him we should never have got to where we are, we should not have had vessels running with 100 and 120 pounds pressure. True it is that the introduction of steel has assisted in the use of those pressures, but we know—at least, all those connected

with steam know—that to this day the Board of Trade have not given their full approval to the use of steel in the same way that Lloyd's Registry of Shipping have done. We have had Fox's corrugated furnaces also to help in increasing the pressure; we also, there can be no doubt, have seen a wonderful improvement in workmanship as regards the manufacture of boilers; no one, like myself, who can go back, I am sorry to say, for almost forty years in connection with boiler-making, can see those large circular boilers now being constructed for these heavy pressures without admiring the workmanship that is put into them. Improvement in workmanship is quite equal to the material that is used, and the result is known to us all. My lord, I could occupy your time with more detail, perhaps, if members would care to listen to me, I would just say a word or two more.

The PRESIDENT. You have spoken nearly fourteen minutes.

Mr. RAVENHILL. Quite right; I can only ask the indulgence of the meeting. Mr. Milton says: "Up to the present time I believe that the reason why the ordinary type of boiler is almost invariably applied for marine purposes is not on account of any restrictions imposed upon new types by Lloyd's Register, or by any other controlling body, but solely because the great difficulties of designing a marine boiler which shall meet all practical requirements better than those of ordinary type have been greater than the inventive genius of our marine engineers has been able to overcome." The necessity and desire has been before us for years that some new form of boiler equally efficient to those that we have had should be introduced; but I must ask Mr. Milton to kindly answer this question when he speaks of locomotive boilers having been removed, I think, after running three months, whether that is the vessel about which he and I had a conversation some months ago; and I think I understood him to say that certain plates in connection with the furnaces were so arranged that one almost wondered any boiler-maker had so arranged them, and that was a source of grief, if nothing else.

Mr. JOHN SCOTT. Would you allow me to say what I intended to say before I sat down—that if any public body, either Lloyd's or the Board of Trade, is disposed to undertake a series of experiments as regards the collapsing of furnaces, my company would be very glad to place the apparatus which they have, and which may facilitate the experiments, at their entire disposal.

Mr. MACFARLANE GRAY. I wish to make a remark in reference to what my friend Mr. Ravenhill has been saying about the Board of Trade. I think we ought to remember what your lordship's words were yesterday about the people who were forward in speaking. I think you will see at once I do not refer to Mr. Ravenhill in this at all, or myself. But you said, my lord, that people were forward in speaking who had not the responsibility, and who had not studied all the *pros* and *cons*. Now, in reference to the Board of Trade, they have been extremely careful, and I think they deserve a little credit for that, for they were perfectly

well aware that they were bringing the animadversion of almost every manufacturing engineer upon them. I partly sympathize with the view Mr. Ravenhill takes, and therefore I speak the more candidly in this way, that the Board of Trade have to do with life—life principally—Lloyd's Registry has to do with property; and we must remember, as the gentleman yesterday explained in reference to bulkheads, that Lloyd's Registry, although they knew that five bulkheads, and the Liverpool Registry, although they knew that five bulkheads, or four bulkheads, or something, were proper, yet they had to abandon it; they could not enforce it because it was a matter whether it would pay, and they could not interfere with the commerce of the country. Now, Lloyd's Registry has to do with that, and they have only to regulate their rules in regard to whether it will pay—"will England make money by this or not?" It is not whether the population in England will be more or less at the end of the year. The Board of Trade have to do with that; they have to consider "Will the end of this year bring in two or three losses of vessels, with 400 or 500 passengers each?" With reference to that, we must remember what Mr. Samuda said yesterday, that there are certain things which cannot be too dearly paid for. Now, life is a matter which the Board of Trade has been appointed to care for, and it is for life principally, if not only life. Therefore they, I think, may be judged leniently—or rather, you ought to try to put yourself in their shoes, as it were, and consider ought they to draw the public into the adoption of unproved materials, or ought they not rather to wait until they can justify such an innovation by pointing to years of satisfactory experience of their fitness? I think that is their position, and they are very willing to sanction lighter scantling for better materials when their qualities have been properly established, but they no doubt wish that the public should see that they did not draw the public, or they did not tempt them to make their boilers too light. I think Mr. Parker will be very ready to admit that he knows that they were acting honestly according to what they believe, and that if they had seen their way that it was safe, what was wanted, they would have been very happy to have acted as Mr. Ravenhill and many others were anxious that they should do.

Mr. Ravenhill. I wish to say, in explanation, and I wish to say it in the strongest language I can use, that I did not wish to impute any other than the most honest of motives either to the Board of Trade or its officials; it was merely a question of judgment, and that was the point which I wished to point out.

Mr. Martell. With reference to an expression that fell from Mr. Macfarlane Gray, I suppose he was alluding to a remark that I made yesterday with reference to the bulkheads of steamships. He says that the Board of Trade are differently situated from Lloyd's Registry, because it is not a question with them the saving of human life, but it is a question of whether it will pay. It was admitted, he says, yesterday, that it

was just a question of money as to their requirements—whether the thing will pay or whether it will not.

Mr. WEST. Mr. Gray said it was stated by the Liverpool Registry, not by you.

Mr. MARTELL. I beg your pardon. No one in connection with the Liverpool Registry made any remark with respect to the subject whatever. Mr. Gray did partly correct himself on that, certainly; but I must take, unless Mr. Gray corrects me, that the remarks which he made applied to Lloyd's Register. I should like to appeal to him now whether he intended that remark to apply to those I made yesterday?

Mr. MACFARLANE GRAY. I do not remember whether it was you or Mr. Rundell. But it was said that a registry proposed to put five bulkheads, and the impression made on me and others in the room was that it was believed by the registry that these five bulkheads would be an advantage, and they tried to introduce them; but they had to be abandoned, because the ship-owners would not stand it at all.

Mr. MORGAN. The shorthand writer's notes will put it straight in the "Transactions." I believe it arose from a remark of Mr. Rundell's.

The PRESIDENT. I must do justice to Mr. Gray. I think Mr. Ravenhill and Mr. Martell are not quite correct. Mr. Gray first said he did not mention anybody as having made the remark about the bulkheads, but he afterwards amended what he had previously said, and referred to a remark that was made by somebody connected with the Liverpool Registry. I think I am correct in that; so really it had not reference to anything you said, Mr. Martell.

Mr. MACFARLANE GRAY. What I understand about the Liverpool Registry or your registry, is, that it is for property. I understand that is its object.

Mr. MARTELL. That is precisely the ground on which I should like to make a few remarks in regard to such an observation as that. Mr. Macfarlane Gray ultimately finished his remarks by saying that he thought the Board of Trade endeavored to do it, and he believed they had the support of the public in the rules that they made with regard to these boilers.

Mr. MACFARLANE GRAY. I said the opposite.

Mr. MARTELL. Now, with regard to the rules made by Lloyd's Registry as to the safety of ships, the remarks I made were these, that they cannot make high-handed rules even like the Board of Trade. If the Board of Trade think that it is necessary to do a certain thing for the safety of life whatever it may be, either for the efficiency of boilers or the efficiency of hulls of ships, they bring it forward by obtaining an act of Parliament, and they take this act of Parliament and place it in the hands of their surveyors, who have only to insist on its being done, and it is done. It does not matter whether it interferes with trade or not; it does not matter to them; they have an act of Parliament for doing a certain thing, and it is done. But with reference to any register soci-

ety, they must have a general opinion among those whose ships they class, that experience has shown it is necessary to do certain things before they can attempt to press these upon the attention of the general ship-owning public. What would be the result if they had rules that were not founded on a sound basis, and the experience of the ship-owning public? The result would be that they would not class their ships, and the registry by which these rules were made, instead of having these ships on their registry, over which they could exercise a certain amount of inspection in yearly examinations, and thereby seeing they were kept in proper order, would merely drive them out altogether and have no control whatever over them; so that instead of doing any good, or being of any advantage whatever, they would be destroying any influence they wished to exert for the benefit of the ship-owning community. Before, therefore, they make any rules making their compliance imperative with regard to the classification of a ship, they feel that there must be a consideration that any rule they insist upon should not unnecessarily interfere with the carrying on of trade, but must be a question with reference to the actual safety of the ship, and if any deficiency said to exist does not interfere with that, then they feel that they can allow ship-owners to exercise their own opinion, and do what they think proper up to a certain point; but immediately it interferes with the actual safety of the ship, then they feel it is necessary, whatever the result may be, to make an imperative rule that shall be complied with before the ship receives a certain class in their books. That is what governs the action of the Registry Society with which I am connected.

Mr. W. Parker. My lord, I must say that I am somewhat disappointed to-night to find that, with the exception of our friend, Mr. Macfarlane Gray, whom we all know as a regular attendant, there are none of the officers of the Board of Trade present with us. I did expect to see some of the officials who have taken an active part in framing the rules of the Board of Trade on the strength of boilers, more especially as they were furnished with copies of Mr. Milton's paper a few days since, in order that they might come prepared to discuss the subject. Now, this paper, I think, my lord, you will see, has been written for the purpose of showing to this institution and to the public at large, and I may add to the Board of Trade likewise, that the rules of Lloyd's Register on boilers are based upon sound principles. A great deal of comment has been made throughout the country, especially on the east coast, with regard to the difference that exists between the rules of Lloyd's and those of the Board of Trade, and this paper explains the reason of this difference, so far as Lloyd's are concerned. The only difference practically between the rules of the two bodies is this, that when you come to thick shell plates, the rules of Lloyd's Register sanction a much higher pressure being carried than that approved by the Board of Trade. As an illustration of this, I may mention, that if the

boilers of the steamer Aberdeen, the engines of which were discussed at our meeting yesterday, had been constructed under the Board of Trade rules, instead of being allowed to carry a pressure of 125 pounds per square inch, which they are granted under Lloyd's rules, they would be limited to 90 pounds per square inch. Now, while our rules allow a higher pressure than has generally been adopted in marine boilers, they provide in all respects for safety, and I think it is only right that controlling authorities in such matters should permit ship-owners to use the highest possible pressure consistent with safety, or, in other words, to realize to the full the economy of fuel which is open to them. As I had occasion to point out recently at a meeting of a kindred institution at Newcastle, we know with approximate accuracy what our factor of safety is required to cover in a marine boiler. The shell is supposed to be a perfect circle, and we know quite well that it varies in strength inversely to its diameter in inches; we know also the strength of the material that we put into the shell, because now that we are using steel we test every plate; and we likewise know quite well, from a number of experiments that we have made on the strength of riveted seams, how to proportion the rivets so as to give the requisite percentage of strength of joint relatively to the plate; so that really in that structure we have nothing to provide against—I mean in a broad sense—but corrosion. We do not require a large factor of safety to cover a plate that we know is safe, or a large amount of margin on a joint whose strength we can so accurately determine; the only thing we require is factor of safety to cover corrosion. Corrosion being constant, whether in a thick plate or a thin plate, Lloyd's rules allow the thick plates to have at the beginning of the life of the boiler a smaller factor than the thin plates, so that at the end of the life of the boiler the thin plates and the thick plates will have approximately the same factor of safety. It is in that respect that the rules of the Board of Trade and of Lloyd's Register differ, and it is a most serious thing, because, as we have seen in the case of the steamer Aberdeen, it means the difference between 90 pounds and 125 pounds pressure. I wish to bring this clearly before the meeting, and, as I said before, it is to be regretted that the leading officials of the Board of Trade, who would have been able to enlighten us in regard to the principles upon which they proceeded in framing their rules, are not here to-night to help us in our deliberations, and enable us to come to some proper understanding in the matter.

Mr. THORNYCROFT. I beg to make a few remarks on this paper. I believe it has been said that the locomotive boiler is unsuitable entirely for use at sea. My firm has had considerable experience with this kind of boiler—in fact, it is the boiler that has been used almost entirely, to the exclusion of boilers of other types. The locomotive boiler is manifestly unfit to work with water where any considerable amount of sediment is formed, because the amount of space in the bottom of the boiler to receive sediment is so very limited. Our experience is this, that

with surface condensing engines the amount of sediment is small, and the limited space for sediment does not seem to interfere with the useful working of the boiler. The boiler has great advantages over the ordinary cylindrical form, because, not providing that space for sediment, it does not require the material to contain that space, and is much lighter in weight. The only trouble we found with that kind of boiler was that the tubes and the tube plate were likely to leak; but that may be more from the use to which we put the boiler than from the inherent qualities of that kind of boiler itself. If the boilers were worked at a more economical rate of working, such as is usual in the mercantile marine, then I imagine this trouble we have to contend against with the tube-plate would not exist. It seems to me that, though not perhaps the best boiler for the purpose where the introduction of weight is of little consequence, it is a suitable boiler to be used at sea with surface condensing engines—perhaps not for long voyages, but there are cases where I think it could be used with advantage when the reduction of weight is very important. I will not trouble the meeting further on this. With regard to some remarks Mr. Parker made as to corrosion and durability of boilers, I would merely say I entirely concur with him that, in considering the life of a boiler, you must allow that the corrosion of the several plates would be approximately uniform in amount, and therefore you do not require to allow an equal percentage of thickness on the thicker plates to what you would on the thinner ones, and therefore the external shell of a cylindrical boiler may be made thinner in steel than would otherwise be required according to some rules.

Mr. MACGREGOR. I would make one or two remarks, my lord, with the permission of the society, relative to the factors of safety that have been spoken of. It appears to me from the remarks of the paper, that provision is made at the commencement of the life of a boiler for the condition the boiler may be in at the end of its life. I do not quite understand what the object of continually surveying boilers is, unless it is to grasp the condition of the boiler at the period of the survey. If the plates are corroded considerably during the previous life of the boiler, then at the survey it is for the Board of Trade, or as it appears also, Lloyd's Registry, to reduce the pressure on the boilers, retaining, as I should have said, the factor of safety the same. I do not see why the factor of safety should vary. If the boiler is reduced in thickness at a certain period of its life, then reduce the pressure accordingly, but retain the factor of safety the same. I might also remark with reference to locomotive boilers, as applied to marine purposes, that I perhaps have used them as much as any person in that particular branch of use, and I have had every satisfaction with them. I do not think that there is a better boiler for marine purposes than the locomarine boiler. Certainly, if you use them with very salt water, that is liable to any considerable amount of deposit, they are not so good, but

it is quite well known now that a number of steamships with surface condensers have very little deposit indeed in the boilers; in fact, some run for several days at sea without any supplementary feed at all, and therefore the water in the boilers is not liable to any considerable amount of deposit. And, moreover, it is possible to arrange locomotive boilers so that they can be examined very minutely, even at the parts specially alluded to, the sides of the boilers. There are one or two arrangements of locomotive boilers for marine purposes in which it is possible to examine them in almost any part and clean them.

Mr. MILTON. I will first deal with what Mr. Macgregor has said with regard to the factors of safety. He thinks it would be a very easy matter to reduce pressures continually as the boilers corrode and get weak. So it would. But that would mean, if we only start with the factor of safety that is absolutely necessary for safety, that after a few months' running of the boilers they would have to be taken out and new ones put in, because the engines or ships could not be profitably run at low pressures if they were first made for high pressures. Very little reduction is possible if you want to make money with your ships. That is the point at issue.

Mr. MACGREGOR. In some instances no reduction is necessary.

Mr. MILTON. If you have a ship first made to go at ten knots, and her trade requires her to go at that speed all the way, and if, after a few months' working, the surveyor comes and says you must reduce your pressure from 70 to 50 pounds, and you can only get eight knots out of your ship with the reduced pressure, you may just as well lay her up at once in a good many cases. But next about the suitability of locomotive boilers for marine purposes. I am afraid Mr. Thornycroft's boilers have never been put into an ocean steamer, a steamer that goes for many months at sea continuously; if they have been, I am certain they will fail. The only locomotive boilers I have known to be tried at sea, and also several of similar type, all have had the same difficulty; they could not be kept in good condition. It is a matter of impossibility to keep boilers perfectly clean at sea, although the engines are fitted with surface condensers. Every Lloyd's surveyor or Board of Trade surveyor knows, and every superintendent engineer knows, that almost constantly when the ships are in port the boilers have to be chipped and scaled, though they are fed from surface condensers. If you have scale accumulate in the boilers you cannot get locomotive boilers to stand. I believe that the reason why the tube-plates of locomotive boilers, when they are run hard, leak so much more than those of other boilers, as mentioned in my paper, is simply the effect of the different changes of temperature. The front part of the tube retains its tightness by being put in a state of compression, and as it is not surrounded by water in the tube-plate, the last three-quarters of an inch of it gets very hot, and every time you open the doors it contracts; the cold air going in there runs direct to the tube, not having to go over a great amount of fire, as it would in the

ordinary boiler, and the tube leaks. With regard to the locomotive boiler I have mentioned, Mr. Ravenhill asked me if this was the boiler that had the defective arrangement of the landings of the furnace-plates. It was. The defective landing gave the engineers a lot of trouble, but it did not interfere with the safety of the boiler at all, and it was not for that that the boiler was taken out. It was taken out simply because it was impossible to run it at sea. The owner of the vessel was an engineer himself; he went the first voyage in her, and was very sanguine about the boiler. All his friends advised him not to put it in, but he was too sanguine, and did, and after three months in the ship he came to the conclusion it was a dead failure, and ordered another boiler at once; and he also came to the conclusion that the failure was not on account of want of attention in any way on the part of the attendants during the voyage, because he was there personally, and had good men under him. The tubes in that boiler gave a great deal of trouble by leakage; every time the door was opened some of them would start leaking, and the salt and water falling on the fire put it out. In that boiler in three months, although there were large water-spaces, the furnace-plates between the stays buckled. The space was the same as we allow in ordinary boilers between combustion chambers, but the heat is much more intense, and the plates get scaled and buckled. It is the intense heat, I believe, that buckles the plates there. Mr. Ravenhill made a slight mistake with regard to Lloyd's Rules; as to furnaces, they are the same, and have not been reduced from what they were when they first came out, and I doubt very much whether they can be reduced safely. Mr. Macfarlane Gray has made some remarks about Lloyd's rules being regulated simply to make vessels pay, and having nothing to do with safety to life. Mr. Martell has answered that very clearly. Vessels cannot pay at all if there is any risk of having boilers explode on their voyages. The only other point was that raised by Mr. Scott, the first speaker, Mr. Scott was speaking of the roundness of the flues, and he quite agreed with me on that, and said that roundness is the all-important point, except when corrugated furnaces are in question. I must say Mr. Scott is right; a corrugated furnace, if it is a little bit out of truth, seems to hold its strength at any rate more than a plain furnace. In testing plain furnaces, the very moment the ultimate pressure is reached, however slow you are pumping, the handle of the gauge falls back immediately, the pressure goes down, and the more you pump the lower the pressure falls. But if we take the case of a corrugated furnace and a plain furnace under the condition of being short of water, supposing they are originally of the same strength, I rather fancy the plain furnace would have the best of it, because for that to sag very much it has to stretch a good deal; a corrugated furnace has a good deal of length and will bag out immediately. We, however, do not anticipate the furnaces getting red-hot. I think there are no more points requiring an answer, and I must say I am very much obliged to the meeting for the way in which they have received my paper.

The PRESIDENT. Gentlemen, there is only one observation that I will ask you to allow me to make, but I feel it is only fair, and my duty, that I should make an observation upon one point. Many allusions have been made necessarily, in the course of this discussion, to the action and rules of the Board of Trade. Now, I wish to point out to you, as I have had many opportunities of watching the action of the Board of Trade in the execution of acts of Parliament, that the purpose Parliament had in view in all that mass of intricate legislation—for there is too much of it, I think, that has been passed upon shipping matters—was the great object of safety; and I can speak from my own knowledge and constant communication with officials of the Board of Trade, that no men are more sensible than are those able officers of the delicacy and difficulty of the duties they have to perform. It is only fair to them that I should state that. They are fully aware of those difficulties, and they have to steer a very difficult course, because they have in the execution of those statutes to bear in mind the object of Parliament, in the first instance, and to interfere the least possible with the progress and prosperity of the trade of the country. We must bear that in mind, and must make some allowance for what otherwise would appear to be to many of us a frivolous interference on the part of the Board of Trade. The duty is an exceedingly delicate and difficult one. I have only felt it my duty, in justice to the officers of the Board of Trade, to make that remark as the result of my observations. I am quite sure you will permit me to offer to Mr. Milton our united thanks for his paper, and to congratulate you, gentlemen, on the very interesting discussion that has ensued upon it.

IV.

CORROSION IN STEAM BOILERS.

By W. J. NORRIS, Esq., M. I. N. A.

[Read at the twenty-third session of the Institution of Naval Architects, 30th March, 1882, the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

The question of corrosion in steam boilers is one that commands the attention of most engineers. It is a standing source of anxiety, causes trouble and delay, and not unfrequently danger to those in charge. To the navy, steamship owners, and steam users, it means great expense, not only in the actual cost of repairs, but in loss of efficiency, by having in a short time to reduce the pressure to compensate for the corrosion.

The rapidity of the internal corrosion of boilers working at high pressure and supplied with water from surface condensers, assuming, as it does, the acute form known as "pitting," has enveloped it with a certain amount of mystery; and the short lives of the boilers of some of the vessels of Her Majesty's navy led to the appointment of the late Admiralty Boiler Commission to inquire into the subject, before whom much evidence of value was given, and several theories advanced to account for this corrosion.

Of these theories the following have received the greatest credence, and which we may perhaps briefly consider:

GALVANIC ACTION.—This theory has its origin in the hypothesis that a great, or rather infinite, number of galvanic circles are formed by the metallic contact of the iron of the boiler with particles of copper or brass from the condenser-tubes, air-pumps, and feed-pipes, carried in with the feed-water, as the two elements, and the water acidulated or alkaline as the exciting agent. This theory will not, however, bear strict investigation, inasmuch as the particles of copper or brass would, by their weight, gravitate to the bottom of the boiler, where the greatest part of the corrosion would ensue; again, although the particles might possibly lodge and rest on the tops of the furnaces and tubes, they could not support themselves on the under side of same, where pitting is found to go on as much in the case of tubes, and more in furnaces than on the top.

These considerations are sufficient in themselves to negative or at least throw doubt on the truth of this theory, without taking into con-

sideration the equivalent wear that must ensue in the air-pumps, feed-pipes, and condenser-tubes; nor can this theory be readily applied to the corrosion that takes place in steam-chests and superheaters; and the reliable analyses hereto appended for reference, and which are copied from the Blue Book of the Boiler Commission, show the presence of nothing likely to account for galvanic action. Had particles of copper or brass been deposited within the boilers, they would have appeared in one or other of these analyses.

In fact, these analyses are somewhat remarkable, as showing the presence of nothing that could account for the corrosion and the rapidity with which it is affected, and we may dispose of the theory of galvanic action as untenable, by referring to the case of the Propontis, mentioned by Mr. A. C. Kirk in his evidence, who states that the vessel was fitted with Rowan's water-tube boilers, ordinary working pressure at sea 120 pounds, had no copper within the boiler, had iron feed-pipes and iron air-pumps, the steam-pipes being of copper and the condenser-tubes brass; but pitting went on with such rapidity that in about nine months a great part of the tubes had to be renewed. From this it is evident that there being no brass in the air-pump or copper pipes to be scored, particles of that metal could hardly have been carried into the boilers.

The next theory that has been advanced to account for the corrosion is that hydrochloric acid is produced within the boiler by decomposition of the water, its hydrogen combining with the chlorides contained in sea-water. If such were the case the water would become strongly acidulated, and if it attained sufficient strength to cause pitting it would have a solvent action on zinc placed therein. Such is not, however, the result of practice, as it will be seen by the analyses above referred to that the water has generally and probably always, except in an isolated case or two, an alkaline reaction. Further, iron when heated with pure water exerts no decomposing action, at least to 400° Fahrenheit, and it has, moreover, been established by careful experiment, that iron when heated with water alone to 356° Fahrenheit yielded no trace of hydrogen.

Another theory that has been received with favor is that of decomposition of the lubricants used for the cylinders and slides. The resulting fatty acids dissolving the brass of the condenser-tubes, and passing into the boiler converts the water into a solution of copper, which acts chemico-galvanically or electro-chemically upon the water and the iron, causing corrosion; but this theory is upset by the consideration that the wear of the condenser-tubes would be in proportion to the corrosion of the boiler-iron, and by the fact that pitting occurs just as much when the condenser-tubes are tinned, and is further negatived by the usually alkaline condition of the water. In well-authenticated experiments it was found that water taken from marine boilers was not dis-

colored by the absorption of sulphuretted hydrogen gas, showing thereby that it held no copper in solution.

These two latter theories are further met, and may be considered as disposed of, by the remarks of Mr. Weston, Admiralty chemist, Portsmouth, in his evidence before the Boiler Commission on samples of water taken from the boilers of Her Majesty's ships Serapis and Crocodile, which are well worth quoting, and are as follows:

"ACIDITY.—Not one sample of water, either of those recently received or those obtained in December, showed any acidity, but, on the contrary, the water was in each case faintly alkaline, that from the hot-well being more strongly so than that from the boiler.

"COPPER IN SOLUTION.—A solution of copper I made for comparison, containing only one in one million parts, showed distinctly the presence of even this minute quantity of copper without resorting to any concentration of the solution, and as no indication was obtained from any sample of water from the troop ships, this metal is certainly not present to any appreciable extent. The condenser tubes in the Serapis and Crocodile are mostly copper, only 400 out of 14,400 in the latter vessel having been replaced with tinned tubes."

And upon these results he makes the following observations:

(1.) That the alkaline condition of the water of these ships clearly shows that acidity is not the cause of the deterioration of the boilers.

(2.) That the absence of copper, in even the minute proportion to which I have tested the water, shows that no injurious action from any solution of this metal by the water in passing through the condenser can take place, and as the copper is not dissolved to any extent, it would appear that tinning the tubes is unnecessary.

It will doubtless be admitted that these results and remarks are very conclusive, and although they apply to vessels of the royal navy, it is evident that they will be equally applicable to those of the mercantile marine.

Carefully and closely examining the above theories, one finds they are based on frail foundations, consequently neither is tenable, nor adequately explains the cause of corrosion in steam boilers, and we now come to the final theory of oxidation, by the presence of free oxygen in the water, derived from the atmosphere.

This latter theory appears to be the true one; the more closely it is examined the more convincing does it, in the writer's opinion, become, and, moreover, it affords an answer to all the results observed in practice.

To show that this is the case, we may now examine this theory and adduce some proof thereof; but, in order to follow the line of reasoning upon which this conclusion is based, it is necessary to glance at, so as to have well in mind, the nature of the elements we have to deal with, namely, air and water.

AIR.—Atmospheric air consists of a mixture of oxygen and nitrogen gases, in the proportion in one hundred volumes of—

Oxygen	20.96
Nitrogen	79.04
	100.00

These gases as composing air are simply mechanically mixed together, and not in any way combined, but each is in its free and natural state, retaining all its properties unaltered and exercising separately and independently its own peculiar functions. Air also holds ammonia and carbonic acid; the former is, perhaps, of but little moment to the subject under consideration; the latter, however, plays an important part, and although it appears to exist in the atmosphere in no definite proportion, averaging only about one in 2,500 parts, it is comparatively very soluble in water.

WATER.—Water in its pure state is a compound of oxygen and hydrogen, formed by the union of one volume of oxygen with two volumes of hydrogen, the two gases being chemically combined, and forming an entirely new substance, a neutral liquid, not retaining any of the distinctive properties of either gas from which it is produced, or able to exercise the functions peculiar to either gas without decomposition. It is incapable of combining with the metals, although it does so with some of their oxides and salts. By some metals it is rapidly decomposed, while it oxidizes others by the aid of atmospheric air, even at the natural temperature. It is a very powerful solvent, liquefying alike both solids and gases, but has no effect on iron except when holding free oxygen in solution, when it becomes a powerful oxidizing agent, and, as a general rule, the more oxygen it holds in solution the more rapidly does it rust or corrode iron, such corrosion being accelerated by increase of temperature, as well as by the presence of dissolved carbonic acid.

Such, then, are the constituent parts and properties of the elements with which we have to deal, and it is in the power of water to corrode iron by the aid of oxygen derived from the atmosphere, and accelerated by high temperature, to which the rapid deterioration of boilers must be ascribed.

Water absorbs and holds in solution all the gases, in volumes varying with its affinity, or the force of its attraction for each particular gas, and for the gases with which we are concerned, it dissolves in 100 volumes, at 0° C temperature, and one atmosphere of pressure, of—

	Specific gravity.	Volumes.
Air	1.00	2.471
Oxygen	1.11	4.115
Nitrogen	.927	2.035
Carbon dioxide, or carbonic acid	1.527	179.67

The law under which the absorption of gases by water takes place is as follows:

That the volume of gas absorbed varies directly with the pressure, the temperature remaining the same; that is to say, the volume absorbed is constant under all pressures at even temperatures. Therefore, in order to find the volume of gas equivalent to its normal or natural tension, *i. e.*, one atmosphere of pressure, absorbed at any given pressure, provided no increase of temperature takes place, we have only to multiply the "coefficient of absorption" by the number of atmospheres of pressure at which the absorption takes place.

The "coefficients of absorption" for the gases we have to deal with are as follows:

Atmospheric air, C = .02471
Oxygen, C = .04115
Nitrogen, C = .02035
Carbonic acid, C = 1.7967

These coefficients, however, are for 0° C of temperature, and the degree of solubility of these gases varies with any increase of temperature, becoming less as the temperature rises, and *vice versa;* but the "coefficient of absorption" for any given temperature within a moderate range may be found by a simple formula, viz:

$$C^1 = C - (xt + yt^2)$$

where

C = Coefficient of absorption at 0° C.
C^1 = Coefficient of absorption at given temperature.
x = Coefficients derived from experimental data.
y = Coefficients derived from experimental data.
t = Temperature.

For the gases we have under consideration these "coefficients" are as follows:

	x	y
Atmospheric air	.00065435	.000013547
Oxygen	.00108986	.000022563
Nitrogen	.00053887	.000011156
Carbonic acid	.07761	.0016424

These "coefficients," as above, have been deduced from careful experiments by various chemists, verified by Bunsen, and will be found in Watt's Dictionary of Chemistry, from which they have been extracted.

Therefore the volume of gas absorbed at any given pressure and temperature, having a tension of one atmosphere, is equal to—

$$CP - (\underline{xt} + \underline{yt})$$

where

P = Pressure in atmospheres.
t = Temperature.
C = Coefficient of absorption.
x and y Coefficients as above.

By the aid of this formula the following tables of the absorption of gases under pressures and temperatures corresponding with the ordinary working pressures of steam boilers have been compiled:

TABLE I.—*Oxygen.*

Absolute pressure per square inch ....pounds..	60	75	90	105	120
Pressure in atmospheres................................do....	4	5	6	7	8
Temperature of mass ....degrees C..	145	153	160	166	172
Volume of gas held in solution in 100 volumes of water.....	10.137	13.627	17.171	19.680	24.372
$\frac{\text{Temperature of mass.}}{\text{Atmospheres of pressure}}$ ....degrees..	36.25	30.6	26.66	23.71	21.5

TABLE II.—*Nitrogen.*

Absolute pressure per square inch.................pounds..	60	75	90	105	125
Pressure in atmospheres................................do ...	4	5	6	7	8
Temperature of mass ....degrees C..	145	153	160	166	172
Volume of gas held in solution in 100 volumes of water.....	5.004	7.740	8.493	10.277	11.953
$\frac{\text{Temperature of mass.}}{\text{Atmospheres of pressure}}$ ....degrees..	36.25	30.6	26.66	23.71	21.5

TABLE III.—*Atmospheric air.*

Absolute pressure per square inch..................pounds..	60	75	90	105	120
Pressure in atmospheres................................do....	4	5	6	7	8
Temperature of Mass ....degrees C..	145	153	160	166	172
Volume of air held in solution in 100 volumes of water......	6.084	8.178	10.306	12.471	14.707
Relative volume of oxygen..	2.129	2.955	3.607	4.364	5.147
Relative volume of nitrogen...	3.955	5.123	6.699	8.107	9.560
$\frac{\text{Temperature of mass.}}{\text{Atmospheres of pressure}}$ ....degrees..	36.25	30.6	26.66	23.71	21.5

It is practically impossible to calculate the volume of carbonic acid that would be held in solution under the above conditions, owing to the quantity of air which passes into the water in boilers, and the relative quantity of this gas forming part of the atmosphere; it is doubtless very large, and careful experiments and analyses can alone decide the volume.

These tables show the volumes of gas having a tension equal to one atmosphere of pressure, at which the water becomes saturated under the working pressures and temperatures of steam boilers of the present day, and would be correct if the steam space of the boiler were filled with the particular gas to which they refer—a condition which is of course unattainable, as it is filled with a fluid much lighter than air or either of its constituent gases; therefore, the air pumped or forced into the boilers by the feed pumps must be subject to two distinct actions which

come into operation, viz: (1) The attraction the water exerts for each particular gas; (2) the force of gravity.

In absorbing air, water separates and dissolves a volume of gas according to the attractive force it exerts towards each of its constituent gases, the force of which attraction is shown by the "Coefficients of Absorption," and is equal to

$$\frac{C^o}{C^n} = \frac{.04115}{.02034} = 2.0225$$

or rather more than twice for oxygen than for nitrogen. This is proved by the fact that air expelled from water by boiling is composed as follows: Oxygen, 34.9; Nitrogen, 65.09; total, 99.99.

Hence to saturate water with air at the ordinary atmospheric pressure, for every 100 volumes in solution $\frac{35}{21} \times 100 = 170$ volumes of air must be passed through it, of which 35 volumes of oxygen and 65 volumes of nitrogen are absorbed, the remaining 70 volumes of nitrogen being rejected.

This fact has an important bearing on the subject under consideration. From it, and the two actions just referred to, it follows that the steam and gases forced into the water will rise and leave the water in accordance with their degree of solubility and specific gravities, which, compared with air as unity, are as follows: Spec. grav. of Steam .625, Nitrogen .927, Oxygen 1.11, $C.O_3$, 1.527.

These specific gravities hold good for all pressures under Marriott's law that the pressure varies as the volume, and, further, that these gases expand an equal degree for equal increments of temperature.

Thus it would appear that the steam will leave the water first, the nitrogen second, and the oxygen last, and that the excess gases will range themselves in the steam space in layers according to their densities, through which the steam will rise as it does in water. This will perhaps be better understood by reference to Plate I, where S = Steam, N = Nitrogen, O = Oxygen, A = Air, carbonic acid being omitted.

In the condition shown, these gases have no tendency to rise, except by a slow process of commingling, by which the oxygen is constantly, but slowly, mixing with the nitrogen, and the mixture of oxygen and nitrogen with the steam, the volume so lost being, however, as constantly replaced from the air forced in with the feed-water.

If this is so, we come to the absorption of oxygen and carbonic acid alone, and table No. 1 shows, with more or less exactness, the condition of the water in steam boilers under ordinary working, and water so charged would be ample, if not more than sufficient, to account for the corrosion.

If, however, we take it that no separation of the gases occurs in the boiler, and deal with it as the absorption of atmospheric air, as shown in Fig. 2, Plate I, we have, even then, a large volume of oxygen, which, as will be seen from Table III, ranges from four times and upwards the

volume of this gas held in solution under the ordinary conditions of the atmosphere, and 0° C. of temperature, for the usual working pressures of steam boilers, with an indeterminate volume of carbonic acid, volumes which being, moreover, aided by high temperature, are sufficient to cause rapid corrosion. The probability is that neither table accurately expresses the condition of the water, but that the volume of oxygen in solution varies in actual practice from the limit of the one to the limit of the other.

Referring again to the drawings, the conditions illustrated may, perhaps, be brought out more clearly by comparing the gases within the boiler to those within a well. In the latter case, as every one knows, carbonic acid gas gravitates to the bottom, and has no tendency to rise, owing to its greater density and weight than the atmosphere, the ratio between the two being as 1 to 1.527. Substituting steam in the boiler for the atmosphere, the cases, excepting of course the higher pressures and temperatures, are similar, the ratios between the gases and steam being as follows:

Steam to Nitrogen:	1 to 1.395.
Steam to Oxygen:	1 to 1.776.
Steam to Air:	1 to 1.584.

These great differences in the specific gravities are sufficient to prevent the denser gases rising, so that the water is constantly charged with gas to saturation, either oxygen and carbonic acid alone or a mixture of oxygen, nitrogen, and carbonic acid.

This question is undoubtedly worth solving, and is one that, assuming the theory of free oxygen in the water, with properly constructed appliances, admits of almost easy solution. The expense of same would probably prevent private firms and individuals from taking up this question, as, apart from the cost of the necessary appliances, there would be the employment of a chemist competent to deal with it, and to make a series of quantitative analyses.

Not being, however, in possession of data derived from experiment, in this direction, let us examine what proofs of the theory can be adduced from actual working.

We have seen that the theory of galvanic action, though possible, is very improbable, and that the alkaline condition of the water negatives the theory of acidity, but does not the perished zinc from the boiler afford some evidence as to the action that takes place? On carefully examining this substance it seems to have changed to an oxide, similar to that formed on the surface of the molten metal when exposed to the atmosphere, and if so, both would be subject to the same tests.

The first test would be that each would be fixed in the fire. On subjecting the perished zinc to this test it was found to be thoroughly fixed; even under the intense heat of a blow-pipe flame it remains unchanged, except that portions of the metal that have not become oxidized melt out, ignite, and burn.

A second test for these oxides is that each would be soluble in hydrochloric acid, and in water acidulated with sulphuric acid. On carrying out these tests both will be found to be dissolved. There is, however, this difference between them, that the oxide from the boiler takes longer to decompose, due probably to the different conditions under which the oxidation was effected, as the oxide formed on the surface of the molten metal is by the oxygen derived from the atmosphere in a rarified state, due to the heat, while that from the boiler is effected under water and under pressure, at a temperature of about half that of its melting point, and is probably hydrated.

It follows from these tests that the process by which zinc perishes in steam boilers is analogous in many respects to the formation of an oxide scale on the surface of the molten metal exposed to the atmosphere, and the question arises, having a high temperature, whence came the oxygen necessary to transform the metal? Surely not from any decomposition of the water or of the salts contained therein, for if any took place, the hydrogen set free would combine with the chlorine of the chlorides contained in sea-water, on account of its intense affinity for this gas, the water would become strongly acidulated, and the zinc probably dissolved; but the answer is, we submit, to be found in the theory of absorption, as herein stated.

It may, perhaps, be worth mentioning that a slab of zinc, say 12 by 12 by 1½ inches thick, is completely oxidized in trips from London to the Mediterranean in about six weeks, while in the home trade similar slabs perish in about eight weeks, under steam about four days per week.

The theory of oxidation by free oxygen in the water provides an answer to the point that the corrosive agent, whatever it may be, must be thoroughly diffused throughout the water, and from the laws herein stated it will be evident that wherever the water is quiescent and coolest, as that portion of the boiler below the fire bars, and where the circulation is restricted, as amongst the tubes, there the pitting will be greatest, and that it will be least where the heat and circulation are greatest, as on the tops of furnaces and combustion chambers, and such, there is reason to believe, is the general experience.

This paper has dealt more particularly with the extent of the aerification of the water in steam boilers, rather than the process by which pitting ensues therefrom, for an explanation of which we cannot do better than refer to the very able paper by Mr. Mallett, in Volume XIII of the "Transactions of the Institution of Naval Architects," wherein the causes are well explained.

In conclusion, the study of this subject points to the remedy for the evil being to supply the boilers with water free from air or gases in solution, and as oil or grease within a boiler can do no good, they should be kept out; if this were done, boilers would, doubtless, last very much longer, and be more profitable to their owners.

ANALYSES HEREIN REFERRED TO.

Analyses of deposit from the boilers of the steamship Carnatic.

Moisture lost at 212°	18.7	18.9
Organic moisture and matter given off above 212°	3.3	3.3
Fat	3.4	3.2
Residue insoluble in hydrochloric acid, sand, &c	1.36	1.22
Oxide of copper	.42	.35
Peroxide of iron	60.93	60.34
Protoxide of iron	5.55	
Sulphate of iron	.67	
Alumina	2.8	2.8
Sulphate of lime	1.03	
Magnesia	.92	
Chloride of sodium	.93	
	100.01	

Analyses of four samples of deposit taken from the boilers of the steamship Carnatic.

	Aft starboard No. 1 boiler.	Fore starboard No. 2 boiler.	Fore port No. 3 boiler.	Aft port No. 4 boiler.
Oxide of iron	42.11	47.0	40.08	40.32
Tallow	10.38	11.18	8.16	8.15
Sulphate of soda	2.13	2.03	2.0	1.74
Muriate of soda	1.31	1.18	2.13	1.64
Carbonate of lime	10.03	8.24	9.60	7.91
Sulphate of lime	2.12	3.0	2.18	1.8
Sand and clay	8.99	6.10	13.12	8.28
Water	22.47	21.27	22.	30.0
	99.49	100.	99.27	99.79

No trace of copper.

Analysis of a sample of boiler from the steamship Elder.

[Specific gravity, 1.12. Reaction, alkaline.]

Sodium chloride	20.633
Magnesium chloride	1.8631
Magnesium sulphate	1.1699
Water	76.334
	100.

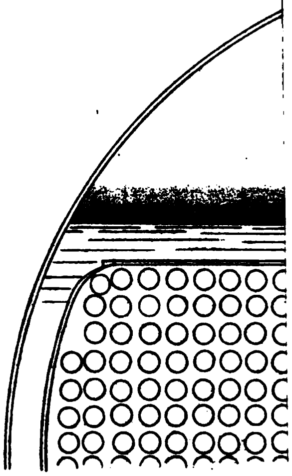
DISCUSSION.

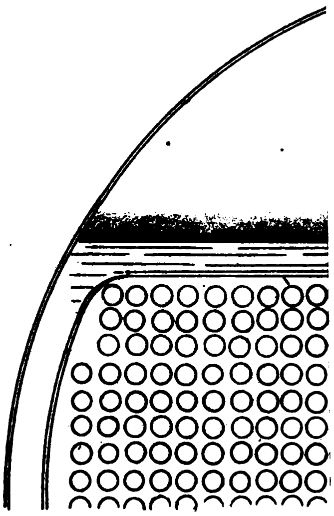
Mr. RAVENHILL. My lord, it is now nearly half past ten, but I do not quite like that we should leave without thanking the author of this paper for the time he has devoted to writing it, and I regret very much that the hour has arrived when but little discussion can take place on it. I will make a remark only upon two points, but not to criticise them. He alludes here to zinc perishing, and gives us his experience of its durability, or, if I may say so, its life, when applied in blocks in boilers where it has been fitted with the view of preserving them from rapid decay. I was informed the other day, when I was on the Clyde, that there are engineers there who hope that they see their way to be able to abandon the use of these zinc blocks, and that by the free use of alkalies

their use will altogether be done away with. I merely mention it as a thing that is looming, and in another year we may have some data with regard to it brought before the Institution. With reference to the writer's concluding paragraphs, he says if all this is done boilers doubtless would last very much longer, and would be very much more profitable to their owners. In that I most cordially agree. It is a thing which everybody connected with steam navigation for years has been driving at, and although we have made very rapid strides we have not yet found ourselves transferred to that happy land.

The PRESIDENT. Gentlemen, I also regret very much that time has stood in the way of a more effectual discussion of this paper; but I beg to say that that accident in no way whatever detracts from the merit of the author, from the great research and from the very great knowledge he has shown in compiling this paper. I will also point to another thing in which I am sure you will all agree with me: It is just one of those papers of so highly technical a character that anybody almost, however able, would find a difficulty in adequately discussing it on the spur of the moment. It would require the most able and experienced chemist really to elucidate many of the questions raised in this paper. I think that the reader of the paper will feel himself that that may be taken as a partial excuse to him for the very limited discussion which his most able paper has failed to bring out. I am quite sure, as I said before, that every credit is due to him for the research which he has shown in this paper and the care which he has taken in preparing it, and therefore I would tender to him, on your behalf, our very cordial thanks.

To Illustr





V.

THE INFLUENCE OF THE BOARD OF TRADE RULES FOR BOILERS UPON THE COMMERCIAL MARINE.

By J. T. MILTON, Esq., *Member.*

[Read at the twenty-fourth session of the Institution of Naval Architects, March 15, 1883; the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

At our last annual meeting I had the honor to read a paper upon "Lloyd's Rules as affecting Marine Boiler Construction." In the present paper I wish to draw the attention of this Institution to the results of the action taken by the Board of Trade in regard to marine boilers; and as I shall have to compare in some respects the Rules of the Board of Trade with those of Lloyd's Register, permit me to say at first that I do this with no spirit of rivalry between the two bodies, for no rivalry is possible between a Government department, compelling their rules to be adopted in all steam vessels engaged in trades requiring them to have passenger certificates, on the one hand, and a voluntary institution like Lloyd's Register on the other, in which it is open to any ship-owner not to have his vessel classed, or to withdraw her from classification the moment that Lloyd's Rules are found to prejudice his business in any way; but I refer to Lloyd's Rules as giving results which are well known to all persons interested in marine-boiler construction and maintenance.

To give some idea of the great influence which the Board of Trade Rules must have upon the commercial marine, I may mention that at the present time there are nearly 1,000 sea-going vessels, of an aggregate gross tonnage of nearly 2,000,000 tons and of nearly 300,000 registered nominal horse-power, holding passenger certificates, and this amount is increasing rapidly; for out of the large number of new steam vessels registered in the United Kingdom in 1882 38 per cent. of the tonnage and 45 per cent. of the nominal horse-power came under the Board of Trade Rules, there being in that year about 114 new sea-going vessels built under these Rules, having a gross tonnage of over 295,000 tons, and an aggregate nominal horse-power of over 41,000.

Seeing what a large proportion of British steam vessels are compelled by law to come under the periodical survey of the officers of the Board of Trade and to have their boiler pressures fixed by the Board's Rules, and recognizing that these vessels are built with the idea of producing a commercial return upon their cost, and that in all cases this return is

largely, and in some cases wholly, dependent upon the economy of their fuel consumption, which is itself governed in a large measure by the steam pressures the boilers are permitted to carry, it is essential that the rules by which the surveyors are guided in fixing these pressures should be framed upon an equitable basis, which, while providing for absolute safety, should in all cases allow the greatest pressures to be carried which are consistent with safety.

It is a comparatively easy matter to calculate the strain produced by any given steam pressure upon the material of which a boiler is constructed when we know its dimensions and scantlings; but it is experience, and experience alone, which can determine the greatest amount of strain which may be put upon a material and still leave a sufficient margin of strength to secure absolute safety. Unfortunately, the only direct experiences which give these data regarding boilers are to be found in those cases in which signs of weakness make themselves apparent, and, as the results of a failure of even a small part of a boiler would generally be very disastrous, such experiences are never sought after, and, happily, very rarely occur; but, on the other hand, we have very many experiences of a negative character, each showing, by the fact of the boilers working continuously with a given pressure safely, that the limiting safe strength is at least not less than that at which the boilers work. As a matter of fact, there are considerably more than 3,000 steamers classed by Lloyd's Register the boilers of which are working, and have worked safely for years, at pressures greatly in excess of those which would be allowed by the Board of Trade Rules, while the practice with all foreign nations is usually to make the boilers still lighter than recognized by Lloyd's Rules.

In comparing the Board of Trade Rules with those of Lloyd's Register, it must be distinctly remembered that the former are intended to apply to all boilers coming under survey, whether the boilers be new or old, and that the longest period for which a passenger certificate is granted is twelve months, while Lloyd's Rules are intended to apply to *new* boilers only; and seeing that vessels with new boilers are classed and retain their class even if their boilers do not become resurveyed for four years, and that the majority of them do not become resurveyed within this period, it is evident that Lloyd's Rules for new boilers must provide an allowance for corrosion, and consequent deterioration of strength, greater than would be required if the boilers were only to be passed for twelve months.

So far, however, from the Board of Trade Rules permitting of higher pressures in boilers passed for one year only than those for which the same boilers would be passed by Lloyd's Register for four years, the Rules of the latter body permit of pressures of from 10 per cent. to 25 per cent. greater being carried than would be allowed by the Board of Trade. Further, of the hundreds of steam vessels coming under the nspection of Lloyd's Register, with their boilers four years old, although

their strength must have then become somewhat reduced, in most cases they are found to be sufficiently strong to be passed for a further period of two years with their original working pressure, and in very many instances are boilers eight years of age and upwards, originally constructed no stronger than now required by Lloyd's Rules, working safely without having had their pressures reduced.

Not only, however, do the Board of Trade Rules insist upon a less pressure being carried in all cases than it would appear could safely be allowed, but these rules, by not being based upon a sound system, severely handicap some particular methods of construction as compared with others, and thus fail to inspire confidence in marine engineers.

For instance, on referring to the rules for cylindrical shells, we find:

It has been represented to the Board of Trade that boilers well constructed, well designed, and made of good material, should have an advantage in the matter of working pressure over boilers inferior in any of the above respects, as unless this is done the superior boiler is placed at a disadvantage, and good workmanship and material will be discouraged. The Board of Trade surveyors have endeavored for some time to take all these points into consideration in fixing pressure, and for this purpose the following rules were prepared, and, at the request of engineering firms, subsequently circulated.

When cylindrical boilers are made of the best material, with all rivet-holes drilled in place, and all the seams fitted with double butt straps each of at least five-eighths the thickness of the plates they cover, and all the seams at least double riveted, with rivets having an allowance of not more than 75 per cent. over single sheer, and provided that the boilers have been open to inspection during the whole period of construction, then 5 may be used as the factor of safety. The tensile strength of the iron is to be taken as equal to 47,000 pounds per square inch with the grain, and 40,000 pounds across the grain. The boilers must be tested by hydraulic pressure to twice the working pressure, in the presence and to the satisfaction of the Board's surveyors. But when the above conditions are not complied with, the additions in the following scale must be added to the factor 5, according to the circumstances of each case:

A	.15	To be added when all the holes are fair and good in the longitudinal seams, but drilled out of place after bending.
B	.3	To be added when all the holes are fair and good in the longitudinal seams but drilled out of place before bending.
C	.3	To be added when all the holes are fair and good in the longitudinal seams, but punched after bending instead of drilled.
D	.5	To be added when all the holes are fair and good in the longitudinal seams, but punched before bending.
E*	.75	To be added when all the holes are not fair and good in the longitudinal seams.
F	.1	To be added if the holes are all fair and good in the circumferential seams, but drilled out of place after bending.
G	.15	To be added if the holes are fair and good in the circumferential seams, but drilled before bending.
H	.15	To be added if the holes are fair and good in the circumferential seams, but punched after bending.
I	.2	To be added if the holes are fair and good in the circumferential seams, but punched before bending.
J*	.2	To be added if the holes are not fair and good in the circumferential seams.
K	.2	To be added if double butt straps are not fitted to the longitudinal seams, and the said seams are lap and double riveted.

L	.1	To be added if double butt straps are not fitted to the longitudinal seams, and the said seams are lap and treble riveted.
M	.3	To be added if only single butt straps are fitted to the longitudinal seams, and the said seams are double riveted.
N	.15	To be added if only single butt straps are fitted to the longitudinal seams, and the said seams are treble riveted.
O	1.0	To be added when any description of joint in the longitudinal seams is single riveted.
P	.1	To be added if the circumferential seams are fitted with single butt straps, and are double riveted.
Q	.2	To be added if the circumferential seams are fitted with single butt straps, and are single riveted.
R	.1	To be added if the circumferential seams are fitted with double butt straps, and are single riveted.
S	.1	To be added if the circumferential seams are lap joints, and are double riveted.
T	.2	To be added if the circumferential seams are lap joints, and are single riveted.
U	.25	To be added when the circumferential seams are lap, and the strakes of plates are not entirely under or over.
V	.3	To be added when the boiler is of such a length as to fire from both ends, or is of unusual length, such as flue boilers; and the circumferential seams are fitted as described opposite P, R, and S, but of course when the circumferential seams are as described opposite Q and T, V .3 will become V .4.
W*	.4	To be added if the seams are not properly crossed.
X*	.4	To be added when the iron is in any way doubtful, and the surveyor is not satisfied that it is of the best quality.
Y	1.65	To be added if the boiler is not open to inspection during the whole period of its construction.

Where marked * the allowance may be increased still further if the workmanship or material is very doubtful or very unsatisfactory.

The strength of the joints is found by the following method:

$\frac{\text{Pitch—Diameter of rivets} \times 100}{\text{Pitch}}$ = percentage of strength of plate at joint as compared with the solid plate. $\frac{\text{Area of rivets} \times \text{number of rows of rivets} \times 100}{\text{Pitch} \times \text{thickness of plate.}}$ = percentage of strength of rivets, as compared with the solid plate.†

Then take iron as equal to 47,000 pounds per square inch, and use the smallest of the two percentages as to the strength of the joint, and adopt the factor of safety as found from the preceding scale:

$$\frac{47{,}000 \times \text{percentage of strength of joint} \times \text{twice the thickness of the plate in inches}}{\text{Inside diameter of the boiler in inches} \times \text{factor of safety.}}$$

= pressure to be allowed per square inch on the safety valves.

This scale seems to provide for all possible methods of constructing cylindrical boiler shells, and if the additions to the factor of safety made by it were such that the ratio of the ultimate strength to the working pressure allowed was in all cases the same, no objections could be raised to the scale. This, however, is far from being the case.

It is generally received that the strength of a cylindrical boiler shell to resist an internal pressure is dependent upon the strength of the material and of the proportions of the longitudinal seams. The pressure

† If the rivets are exposed to double sheer, multiply the percentage as found by 1.75.

on the ends of the boiler, being mostly borne by the stays, causes very little strain upon the circumferential seams, and so long as these seams are tight no more is required of them; the design of these seams, whether lap, single strapped, or double strapped, single or double riveted, with punched or drilled holes, having absolutely no effect upon the ultimate strength of the shell. There are, however, no less than six provisions in the scale as to the design of these joints, and five as to the method of making the holes in them, some of these provisions involving a possible reduction of 12 per cent. in the working pressure. Curiously enough, although these eleven provisions are made, no notice whatever is taken of the proportions of the diameter and pitch of the rivets in regard to the thickness of the plates, the most important points so far as the efficiencies of these joints are concerned.

With regard to the longitudinal joints, there can be no doubt that if the holes are punched the plate is weakened to a somewhat greater extent than it would be if they were drilled, and possibly the plate may be left a little stronger if it is holed after bending than if holed before bending; but there can surely be no justification whatever for the provision A in the above scale, the holes in each case being required to be fair and good and drilled after bending. When we consider the rivets, however, experience has shown that rivets in punched holes are certainly not weaker than those in drilled holes; yet these rules, by requiring the same sectional area of rivet as net section of plate in all cases, or by reducing the pressure in proportion as the rivet area falls short of this, allow a less strain to be borne by rivets in punched holes than by those in drilled holes of the same size; a difference of 10 per cent. being made in this way by the provision D.

It would be impossible, in the limits of this paper, to exhaustively criticise the whole of these twenty-five conditions. I will here only further remark, that the condition V victimizes double-ended boilers to the extent of from 6 to 7 per cent.

The Board of Trade Rules for furnaces, again, give a variety of different pressures for flues of the same diameter, length, and thickness, according to the method constructing the longitudinal joints, a furnace with a welded seam being allowed to work at a pressure 50 per cent. in excess of that which would be allowed if the joint were lapped and single riveted with punched rivet holes. In these rules, as with those referring to the circumferential joints of cylindrical shells, the method of making the holes in the plate is deemed to be of great importance, while the proportions of diameter and pitch of rivets to the thickness of the plates are again ignored.

Now, with the same dimensions and furnaces, and with the same strength of materials employed, it is well known that the strength of the structure to resist collapse depends upon the *roundness* of the flue, and not upon the method of joining or holeing the plates. Actual measurements of hundreds of flues, constructed by all the engineering

firms throughout the country, have been taken by Lloyd's surveyors in the ordinary course of their duties, and the results show that not one method of making the joints gives invariably better results as regards roundness than others, some of the flues most out of truth being welded or fitted with double-butt straps.

Lloyd's Rules with regard to furnaces would permit in some cases of more than 70 per cent. greater pressure being carried than would be allowed by those of the Board of Trade; and in no case yet have Lloyd's Rules, out of the thousands of boilers made in accordance with them, being proved to be too light, the whole of the cases of collapse of furnaces which have occurred being clearly due to overheating from various causes.

The Board of Trade Rules in respect to furnaces do not absolutely prevent high pressures being carried in the same way as their rules for shells, because the strength of a furnace can be increased without thickening the furnace plates by fitting anti-collapsing rings, but all engineers who have the responsibility of keeping in order boilers fitted with these rings feel so strongly upon this point, on account of the anxiety and trouble these rings incur, that nothing but sheer necessity can induce them to have them fitted.

Again, the Board of Trade Rules for thickness of flat plates, assume their strength to vary directly as the square of their thickness in sixteenths + 1, and inversely as the surface supported —6 square inches, a law which cannot possibly be true. Moreover, if the co-efficients employed are suitable for the pitches and pressures usually employed with thick plates, this rule must give results dangerously weak when applied to thinner plates with smaller pitches; while, if the co-efficients are suitable for the thin plates, it must seriously reduce the pressures allowed to be borne by thick plates. If any difference in point of original strength is required in the cases of thick *vs.* thin plates, surely the thin ones should be made relatively stronger, since they would be more weakened than the thick ones by an equal amount of corrosion.

These rules for flat plates, like those for furnaces, do not absolutely prevent high pressures being carried, since it is possible to meet them by pitching the stays very closely together; but this close pitching of the stays, like the fitting of anti-collapsing rings to furnaces, seriously interferes with the continued efficiency and safety of the boiler, by rendering it extremely difficult, and in some parts impossible, to obtain access to the various portions of the boiler for purposes of inspection, cleaning, and repair. The rules for shells, however, offer a complete obstacle to further progress in marine engineering, as the thickness of plates now rendered necessary by them to enable existing pressures to be carried is as great as can be properly closed and riveted together with our present appliances; and although powerful hydraulic riveters are employed for this work, it must be remembered that with our pres-

ent form of boiler it is necessary for at least one circumferential seam to be riveted by hand.

In the case of steam vessels built under Board of Trade inspection, and originally intended to receive a passenger certificate, the hardship of these rules, although serious, is not so directly felt as in those cases in which existing steam vessels, through changes of commercial activity, the opening of new trades, &c., require to carry more than twelve passengers; because in the one case the owners from the first have been aware of the disabilities to which they would be subjected, and the machinery has been designed to work at the relatively low pressures; but to the owners of the other vessels it must appear to be not only absurd, but also despotic, that the boilers will only be allowed to carry a fraction of the steam pressure for which the machinery has been designed, and for which both boilers and engines have proved to be perfectly safe. A large reduction of pressure in a compound engine proportioned for a high pressure means a considerable loss of efficiency, and in the case of low-powered steamers it may often happen that this reduction of pressure and power, instead of rendering the vessel more safe, may, by rendering her less manageable, actually tend to increase her chances of succumbing to heavy weather.

It will be within the recollection of many ship-owners and others here that a few winters ago considerable anxiety was felt as to the safety of many steamers then engaged in the Atlantic trade, solely on account of their being of low power.

I have referred in the early part of this paper to some figures showing the immense interest this country has in the steam commercial marine, and have shown that the Board of Trade Rules press unnecessarily heavily upon its prosperity. It is very difficult to estimate the monetary loss annually inflicted upon the country by these rules, but assuming that the average steaming time of all sea-going passenger steamers is 250 days per annum, and that the boilers of these vessels are capable of being worked safely at pressures 20 per cent. greater than those actually allowed, and that this 20 per cent. increase of pressure would give about 4 per cent. increase of efficiency, figures which are all well within the mark, then at least 300,000 tons of coal are burned per annum in these passenger steamers wastefully.

Assuming that the average cost of this coal is £1 per ton, and that the average loss in freight through having to carry this coal instead of cargo is £1 per ton, we see that these rules cost the country more than half a million pounds per year.

Having pointed out some of the great hardships at present inflicted by these Rules, I can only, in conclusion, express the hope that the president and council of this institution may agree with me that this is a subject in regard to which they might with great public advantage bring their influence to bear upon the authorities of the Board of Trade, with a view to having these Rules placed upon a satisfactory basis;

and, if I may make a suggestion on this point, it would be to the effect that in revising the Rules, those engineers of eminence, who have had large experience not only in making but in the upholding and running of marine machinery, should be invited to give their experiences, as these would be of the greatest value in framing rules for minimum strength.

DISCUSSION.

Mr. MACFARLANE GRAY. My lord and gentlemen, I need not tell you that I do not represent the Board of Trade. I know very little about the Rules. The rules belong to another department in the Board of Trade than that with which I am connected; and where there are different departments in a concern, it is sometimes better for those of one department not to pay too much attention to the affairs of another department. I myself, therefore, have not paid very much attention to these Rules, and if I this morning make any mistake in interpreting them, you must forgive me. The remarks that I have to make, then, are simply those which have occurred to me as one having a little knowledge of these things. Looking at Mr. Milton's paper, it seems to me that it expresses the influences of the Board of Trade Rules upon Lloyd's surveyors more than upon the commercial marine. If you turn with me to the paragraph beginning: "It is generally received (this is the first paragraph containing his attack) that the strength of a cylindrical boiler shell to resist an internal pressure is dependent upon the strength of the material and of the proportions of the longitudinal seams." This, then, is to be the basis of our rules, if this body is established to make new rules, and if Mr. Milton has his way. Now, is this the case? It seems to be so, by describing a circle and by drawing a boiler longitudinally, that the circumferential seams are, in every case, if the joints are the same, double the strength of the longitudinal, that is, that they have 50 per cent. to spare. If you ask the boilers themselves, what do they say? Are they not to have any voice upon the matter? I wish I could have taken you with me to a dock in London a few years ago—I am sure Mr. Parker saw it—where you could have seen the boilers of the screw steamer the Thames. In those boilers a circumferential seam went—I am within an inch or two of it—in one instance, say, 100 inches; there was a seam-rip of 100 inches right across the boiler. Now, how would Mr. Milton explain that? Did any of us ever hear of a longitudinal seem of a boiler having 100 inches of seam-rip? Where would the boilers have been if they had been at sea under the normal pressure? Fortunately, on this occasion the pressure was only 17 pounds to the square inch instead of 70 pounds. If it had been 70 pounds—and it might come about to be 70 pounds and that very thing to happen—where would that boiler have

been? Now, I think this ought to dispose very fully of this statement that the strength of a boiler shell is dependent solely upon the strength of the material and of the proportions of the longitudinal seams. I say that it is not; that practically the circumferential seams of a boiler are far more strained than the others, as any man knows who has examined the bottom of a boiler and has seen where the leaking goes on. It is the circumferential seams, it is not by the hydrostatic pressure, nor the normal uniform pressure all through. It is the unequal heating of the parts of the boiler, and the boiler must be made strong enough for that. You may say, "Well, that is only one case." Only one case! Well, you would not require to go very far to find another. While the engineer was looking at this one, he could not understand it. He had heard a strange noise; he thought some one had got a box of safety-matches and was striking them all around him. He heard the noise and could not make out where the sound was coming from, until he saw the boiler actually opening before him. Then, again, he told me, "It was just as if another fellow had got another box of matches behind my back and was striking them there; it was the other boiler going the same way." That was his account of it. He found it was the other boiler going the same way at his back. It went at 17 pounds pressure. Now, the cause of that was that they had run in some cold water at the bottom of the boiler; they had run it in from the sea. They were coating the boiler with cement and heating the boiler to dry it, and had opened the sea-cock and allowed the water to run in from the sea. They, therefore, had a body of cold water lying in the bottom of the boiler, and by and by when the pressure rose to 17 or 18 pounds, away went the boiler in that way. Now, that did happen there, but you may say it could not happen at sea. We did not expect that to happen there. We do not provide specially—we cannot, I suppose—for everything we have to meet in regard to making boilers strong enough. Such an accident might occur at sea in this way: If one of a set of boilers has been for some reason or other laid off, and it is connected again, and if some fool opens the stop-valve, the communicating valve between the two boilers, while a large quantity of cold water is lying at the bottom of the boiler, and lets in at once the full pressure at which it is intended to work, where will the boiler go? It has not happened yet, and these boilers are said to be quite safe. I know they are perfectly safe until some accident of this kind happens. We could all name boilers that were perfectly safe, and believed to be perfectly safe, until there was something about the opening of a stop-valve, or a rivet, bag left in a stop-valve, or something else of that sort, which caused an accident. Now, we cannot tell when the mistake will happen, but that has happened already, and I say that I have shown, I think, from that one example, that it is quite a mistake to make that statement that the strength of a boiler depends upon its longitudinal seams only, and that therefore you need make the circumferential seams only half the strength. I do

not dispute at all that the strength one way against internal pressure is double the strength of the other. The longitudinal seams have equally over the whole length about the same strain, whereas the circumferential seams may have even almost compression on one side of the boiler and a very inordinately increased amount of tension upon the other. Now, that must have been the case with regard to that boiler of the Thames. Mr. Milton ridicules your having these provisions for the circumferential seams. Of course, I do not know whether Mr. Milton is making a mistake to suppose that the additions for the circumferential seams are to be added after the corresponding additions for the longitudinal seams have been added.

Mr. MILTON. They are.

Mr. MACFARLANE GRAY. They are not at all. The rules do not say that the one is to be added to the other.

Mr. MILTON. Read the rule.

Mr. MACFARLANE GRAY. It is simply insulting the surveyors to imagine that they would do it. If you are making an allowance for a chafed rope, and if you find that at a certain part of the rope there is, say, 20 per cent. of it gone, and you make an allowance for that 20 per cent., then in passing over the rope, you find another part of it where it is 10 per cent. gone, Mr. Milton fancies that they are so foolish in the Board of Trade that they would add the two and say, "That is 30 per cent." That is nonsense; they do nothing of the sort.

The PRESIDENT. Mr. Milton will have the opportunity of replying, but I must ask you to be a little cautious in the phrases you use.

Mr. MACFARLANE GRAY. I should be very sorry if I have said anything wrong or out of taste. He says in the paper:

Curiously enough, although these eleven provisions are made, no notice whatever is taken of the proportions of the diameter and pitch of the rivets in regard to the thickness of the plates.

I am not aware of that. I think they are the usual proportions of rivets to plates, and the rules distinctly tell that where there is any boiler with unusual proportions the surveyor or the builder has to apply to the Board, and that unusual proportions come under a different rule altogether. I think that is about all in that paragraph that I need notice. Then in the next paragraph we have this: "With regard to the longitudinal joints, there can be no doubt that if the holes are punched the plate is weakened to a somewhat greater extent than it would be if they were drilled." He says: "Possibly the plate may be left a little stronger if it is holed after bending than if holed before bending; but there can surely be no justification whatever for the provision A in the above scale, the holes in each case being required to be fair and good, and drilled after bending," and he goes on to complain of the 10 per cent. in D. That refers to the provision for holing or punching before bending. Now, remember, I have nothing whatever to do with these rules—nothing whatever. I do not know how they were made, but the

remark I make will, perhaps, explain why they are so particular about this, against bending after holing. I remember that about that time a surveyor, in inspecting the plates in a first-class boiler-maker's place where they were making a lot of very large boilers, looked at a heap of plates in the yard. He said to the foreman, "Are these condemned plates?" "No; these are not condemned plates." "Why, they are cracked at that seam!" "Cracked! Nothing of the sort. We have sent back four or five plates already, but these are all right." Now, on examining the plates it was found that by laying the plates outside, the exposure to the weather had shown, what had escaped the strictest examination before, that several of the plates were gone this way [describing on the board]. The plates were an inch and an eighth in diameter. Taking one of the holes, *there*, that plate had been bent after being punched, and it was cracked between the holes for a dozen holes in succession, and quite a number of plates were found in the same way. I have no doubt that many plates of that kind are put into boilers and the defects not seen at all. Now see what happens to it. They put on a covering strap, or put another plate over that—and remember this one is cracked—and then you put in your rivet, and that is where the crack is, and I have Mr. Milton's own word for it that the strength depends upon the longitudinal seam. *That* is the longitudinal seam. The crack is half through because it was cracked by being bent after being punched. Now, I think that that would be an element of danger in a boiler, and I know that it was a matter of serious consideration with regard to boilers at that time by the Board of Trade. I think they therefore demand from you some consideration for having made, what Mr. Milton calls, this very foolish rule—this ridiculous provision D; because if you recognize the importance of legislating against what would induce you to bend these after punching them, or after drilling them, then all that Mr. Milton has said about provision D, and all that second paragraph, goes away; it must be entirely swept out. I have only attempted to read this coming up in the cab this morning, and I may not understand what Mr. Milton's meaning is, but I think I am treating it fairly. I need only further remark this: Mr. Milton says that the condition "V" victimizes double-ended boilers to the extent of from 6 to 7 per cent. I do not know whether he chooses the word "victimizing" because it begins with V; perhaps that suggested the word, but I cannot see the appropriateness of it. Now, surely, we all know that the strains upon the circumferential seams upon the bottom of a double-ended boiler are something very different from those upon a single-ended boiler. There is a want of elasticity in a double-ended boiler, especially with through furnaces. If you put through furnaces, perhaps making them in one plate from end to end, and having these furnace plates come within, say, five or six inches of the side of the boiler, then there is an unequal expansion and contraction—a hot furnace and a cold boiler shell; this is one of the grievances, and it is a grievance in this boiler, that, because it

nad that extra strain put upon it, you have actually given it 6 or 7 per cent. more strength to stand it. He says that victimizes it, because you make it stronger when it has more to do. I do not know what the meaning of victimizing is, for 6 or 7 per cent. is rather under the extra work it has to do.

The PRESIDENT. I am bound to point out, Mr. Macfarlane Gray, that you have reached the limit of time usually allowed to speakers.

Mr. MACFARLANE GRAY. My lord, I could go on in the same way, I think, for every part of this paper.

Sir EDWARD REED. I rise to order. This paper involves one of the broadest and severest attacks upon a public department that I have known in this Institution. Mr. Macfarlane Gray, it is true, has disclaimed all responsibility for that department, and does not speak in its name; but, as he is an officer in the department, I venture to suggest whether the meeting would not allow Mr. Gray the necessary scope in point of time for replying pretty fully to the paper.

The PRESIDENT. I quite felt that myself; but still I am here to enforce the rules, and I only called Mr. Macfarlane Gray's attention to the rules. I am quite sure the meeting will allow him that latitude which I am perfectly willing to grant. I am acting simply as your servant in these matters.

Sir EDWARD REED. My lord, it has been the usage for the president, when a question of this sort has been raised, to take the sense of the meeting; that is why I rose.

His LORDSHIP having put it to the meeting that Mr. Macfarlane Gray should be allowed to proceed, it was so resolved.

Mr. MACFARLANE GRAY. Then, my lord, in the money paragraph—what I may call the "money article" of this paper—Mr. Milton draws our attention to what he makes up to be nearly half a million of money annually which might be saved. Now, there is a lot more money that might be saved; there are a number of millions of bullion which very many, who do not know anything about it, think lie uselessly in the Bank of England when it might be in circulation. But the money is there for a certain purpose; it is for safety that it is there. If this also provides safety, perhaps there is a justification for this being so. Now, let us see whether Mr. Milton is right with regard to these 300,000 tons. In the first place, he has assumed that every owner is anxious to go into 20 per cent. higher pressures. I question that. He has also assumed that every passenger is anxious that the pressure should be increased 20 per cent. in the engine. I question that. And I make this statement, that I think that if to-day the pressure were increased 20 per cent., no more than one-fifth of the number of vessels here would be increased that 20 per cent. of pressure. Now, if we take it as one-fifth, what does it amount to? As it stands, it comes to something less than $2\frac{1}{2}$*d.* per head per annum, and that would reduce it to less than a halfpenny per head per annum; that is to say, that for less than a half-

penny a year the public can have this security that the boilers that carry them are too strong as pronounced by a rival institution. Now, is that, or is it not, worth a halfpenny per head per annum? Well, my lord, I will not say more, because it really is not fair to the department to which I belong that I should presume to have spoken here this morning, because I believe that those connected with it would be able to make a far better reply than I have made; but still I think there is something in what I have said.

Sir W. SIEMENS. My lord, when gods fall out mortals must suffer, and I present myself to-day here as one of those sufferers, in the character of a ship-owner. Nine years ago I had a ship constructed for cable purposes, the Faraday. I took great care myself of all the arrangements necessary to make that ship efficient for laying and picking up cables, but as regards boilers and engines I relied entirely upon the Board of Trade Rules and Lloyd's Rules as they then existed, my instructions being simply, "Make the boilers as safe and the engines as efficient as they can be made." The result was undoubtedly a success. That ship never failed to do its arduous duties, being out on the Atlantic sometimes in the winter months for weeks together engaged in very rough work. But in the course of a few years the Board of Trade Rules were altered, and it was intimated that these boilers were really no longer sufficient under the new rules laid down. We had the boilers carefully inspected after each voyage, and they were pronounced both by the Board of Trade surveyors and Lloyd's surveyors to be in perfect condition. Nevertheless, in order to induce us to fall in with the new rule, the pressure was reduced after each voyage 5 pounds, and we would very soon have been in the happy condition of depending upon our sails. The operations that have to be carried on on board this ship are so important that I decided to have steel boilers put in, at an expense of £10,000, although the inspection of the boilers proved that they were in perfect condition. Now, this is a hard case to me as a ship-owner, because if the two authorities had been agreed regarding the rules I should not have been put to this expense, and have had to sell good boilers second-hand, which have gone into other ships not requiring to be under the rules of the Board of Trade. As an engineer, and one interested in the manufacture of materials, I should say that the Board of Trade rules give us excessive thicknesses—especially in cases where first class material is used. I see that certain strengths of iron are assumed, not according to the tests this iron can stand, but it is to be taken as equal to 47,000 pounds per square inch in the one direction and 40,000 pounds in the other. Now this seems to involve a condition of things which does not exist—that of all materials being equally good. I would rather say, have each plate, whether it be steel or iron, tested, and if it does not stand an elongation of 20 per cent. in a bar 8 inches long, it ought to be considered unfit for being put into a boiler. Mr. Macfarlane Gray has given us his reasons why

the Board of Trade insist upon a thickness of plate exceeding, as he admits, the necessities of the case. He has given us an instance where, at a pressure of only 17 pounds, a plate had cracked all along the seam, although the boiler was expected to stand 70 pounds. But I fail to follow Mr. Macfarlane Gray in his argument. The forces that come to bear (viz, the strain) upon the material through expansion by heat increase enormously with the resisting power of that material, and therefore the very fact which Mr. Macfarlane Gray has brought before us appears to me to tend rather in the direction of avoiding excess of thickness, because with excess of thickness you get always, whatever be the nature of the material you employ, an inferior plate. But, I say, have sufficient thickness, and be more careful regarding the quality of the material which is employed. If the material of which that boiler was constructed had been such as would elongate 20 or 30 per cent. before breaking, I am quite certain that the strain put upon it by expansion would have had for its results simply a local elongation, but there would have been accommodation; whereas rigid material has no accommodation, but must yield when, through local action at any one point, the strain put upon it exceeds its absolute strength. This, therefore, appears to me to furnish a very powerful argument in favor of a material of high ductility, and the fault I would find with all these rules is that that factor of ductility is not introduced as part of the factor of safety. Now, Sir Joseph Whitworth, who has given much attention to this subject, has laid it down as a rule that the value of a material is composed of the sum of its ultimate strength and of its yielding power under strain. I believe that that is a very excellent rule. Therefore if the factor of safety of 5 is none too much for a material that would break when extended 10 per cent., if it could be made to extend 20 per cent., the factor 5 ought to come down to the factor 4; and you would get with that factor at least as much safety as you would get with the factor 5 and the lower extensibility of the material. This is a question which I hope will be very fully and carefully discussed, and I trust that it will lead to a rule in which both the Board of Trade and Lloyd's surveyors concur, in order to prevent the recurrence of such incidents as I have given you.

Mr. MacColl. My lord and gentlemen, in making a few remarks upon this paper, I shall begin by expressing my personal belief that the effect of the Board of Trade Rules which have been in existence for the last nine or ten years has been to raise the whole tone of boiler-making in this country, and they have done so, not, I think, by improving the better class of boiler-making, but by leveling up, by raising the quality of the work done by those who were previously in the habit of making inferior work. We have all seen boilers made before the existence of these rules, whose condition was simply absurd, double-riveted longitudinal seams which were no better than single-riveted seams, which were only double-riveted in the sense that they had two rows of rivets,

but with regard to strength they were only single-riveted. In making these remarks regarding the effect of the Board of Trade Rules upon boiler-making, I do not lose sight of the fact that a little over eight years since I had the honor of reading a paper before a kindred institution, the Institution of Engineers and Shipbuilders in Scotland, criticising these rules; and in making the statement I have just made I do not forget that the principles which I then thought were wrong in those rules I still think to be wrong, notwithstanding what Mr. Macfarlane Gray has told us to-day. I do not believe, and I do not think that engineers in this country believe, that a double butt-strapped and double-riveted transverse seam increases the strength of the boiler over a double-riveted lap seam; and this is proved by the fact that the ideal boiler, as I may call it, is not at present the boiler of this country, nor the boiler that is made to any large extent in this country. I think we shall find that the usual practice in boiler-making is to lap and double-rivet the transverse seams, and to have the longitudinal seams either lapped and treble-riveted or double butt-strapped and double-riveted—unless, perhaps, in a very few exceptional cases, the other system of double-butt strapping and double-riveting the entire boiler has been adopted. Mr. Macfarlane Gray has to-day given us a reason why we should go in for such a very expensive method of making our boilers. But his reason is not one which I think we should adopt. I do not think the boiler he spoke of would have been a better boiler, or would have stood better, if it had had double-butt-strapped transverse seams. I do not know all the facts of the case, but I should judge that if that boiler had not been rigidly stayed from end to end, and the furnaces had a little elasticity, it would not have fractured at the transverse seam. Upon this point I feel satisfied that the Board of Trade made a great mistake in insisting upon boilers being made too rigid, and I think over-staying has just the same effect on a boiler that tight-lacing has on a lady. The one is ruinous to the health of the boiler just as the other is ruinous to the health of the lady. I believe if the Board of Trade would allow us to put fewer stays, particularly at the lower parts, we should make much better boilers than we do. I have no hesitation in saying that. I quite agree with Mr. Milton that most of these additions for different methods of making the transverse seams should be wiped out of the rules, and that it should be considered that lapped and double-riveted joints in the transverse seams are just as good as anything that can be made. I find, however, that if we take steel—and that is no doubt the material, not of the future but of the present; I do not know what the practice is in London, but in Liverpool there are hardly such things as iron boilers being made, and the exceptions are only made at the express wish of individuals—but taking steel boilers, I find that with a thickness less than nine-sixteenths, the Board of Trade, instead of giving us less pressure than Lloyd's, give us more; and if the thickness is between nine-sixteenths and three-fourths, Lloyd's and the

Board of Trade practically agree, but if the thickness is over three-fourths, Lloyd's give us an advantage of something like 4 per cent. Now, in view of this I do not see how all Mr. Milton's remarks about the effect of these rules can be correct. I have not taken the trouble to look into the matter in connection with iron boilers, because I do not, as far as I can help it, have anything to do with them. You will have noticed, no doubt, that the thinner plate gives, according to Lloyd's Rule, less pressure in proportion. That is just quite in opposition to what Dr. Siemens has just told us to be the correct thing. He has told us that the thinner the plate the greater its proportionate strength—at least I took that to be his meaning; and the sketch made by Mr. Macfarlane Gray is a proof that a thin plate is better than a thick one. Lloyd's have told us to-day why they allow a less proportionate pressure for the thinner plate. From Mr. Milton's comparison of the two rules it appears that boilers are intended to run for four years without a survey. Well, I only express my own opinion, but I think that principle is wrong. No boiler ought to be constructed and no provision ought to be made for the running of a boiler for four years without a survey. My own opinion is that every part of the machinery of a ship ought to be surveyed every twelve months—boilers and everything else. This provision is also made because it is assumed that corrosion is to go on in the boiler. Now, as a matter of fact, the question of corrosion has been thoroughly mastered, at least by some people, I may say; and there are gentlemen in this room to corroborate me when I say that boilers have been built for eleven years, and have been running all that time, and you will not find in the whole of the inside of these boilers one speck of corrosion. Now, if this is possible in one case it is possible in all, and if it is possible in all, why should Lloyd's saddle us with additional thicknesses to provide for something which need not occur? Even if corrosion did occur, I do not see that it is right that the original structure of the boiler should be saddled with an increased thickness to provide for something which may afterwards happen. I think if corrosion does go on to a great extent, as is anticipated, the pressure of the boiler ought to be reduced, and that, in order to provide for this, it ought to be the duty not of Lloyd's, but of the ship-owner, to make the provision. Then there is another thing to be considered. If we take a shell only three-eighths of an inch thick, and assume that it loses one-sixteenth of an inch by corrosion, how much does this amount to? One-sixth—17 per cent. of its original thickness; and if it is made with a double-butt strap, there is no reason why its original strength should be reduced one single pound, because the body of the plate only would be reduced in thickness, while the weaker part, the joint, retained its original strength. While the Board of Trade Rules are not all that can be desired, neither is the basis of Lloyd's Rules quite so sound as one would wish. Now, as regards the pressure allowed on furnaces. The Board of Trade Rule for furnaces is a very

curious thing. There is no doubt about that; still, the effect of both rules is practically the same, as the compressive strain is limited to 4,000 pounds to the square inch, or something like that, I am not sure of the figure, but there is a limit. At any rate, you cannot exceed a certain compressive strain, so that, practically, the thing comes to be about as broad as it is long, taking one rule with the other. In any case, in making a furnace, a thirty-second or a sixteenth of an inch thicker is not a very serious matter. Then as regards the rules for flat plates, I shall pass on, leaving that, because I do not think there is much in it. Lloyd's allow nothing extra for steel, but the Board of Trade do; practically, however, for steel, excepting for thick plates, both rules come to pretty much the same thing. Further on, Mr. Milton says: "The rules for shells offer a complete obstacle to further progress in marine engineering, as the thickness of plates now rendered necessary by them to enable existing pressures to be carried is as great as can be properly closed and riveted together with our present appliances." I have already expressed my own opinion as to these rules; but if we use steel we can make, by the Board of Trade Rules, a boiler to carry 150 pounds in a diameter of 10 feet, with a thickness of less than an inch. And no practical man here would say that this is a thickness that could not be dealt with by ordinary machines, and even according to Lloyd's Rules the thickness would only be 4 per cent. less than that. Mr. Macfarlane Gray has touched upon the last part of the paper, and I agree with him in saying that it involves a great fallacy; I mean the loss to the country of half a million per annum. The whole calculation is based upon the assumption that the machinery of existing steamers has been designed for 20 per cent. higher pressure than is at present used, or, in other words, that the pressure has been reduced 20 per cent. I do not think that is the case at all. I think that, as a rule, the pressure which boilers carry is the pressure for which the machinery was designed, and if we increased the boiler pressure 20 per cent. we should find that the machinery was not capable of standing it. It assumes, as Mr. Macfarlane Gray told us, that owners are anxious to increase pressures, and I do not think this is the case. At any rate, I can speak for Liverpool, where the pressure is now about 90 pounds; we are making boilers to 95 pounds or 100 pounds, but the general pressure is 90 pounds.

Mr. R. H. Andrews. My lord, I should like to call attention to the last paragraph of this paper. Mr. Milton there says that he hopes "the president and council of this Institution may agree with him that this is a subject in regard to which they might with great public advantage bring their influence to bear upon the authorities of the Board of Trade, with a view to having these rules placed upon a satisfactory basis." I would suggest that if ever the officers of the Board of Trade and Lloyd's Register do put their heads together to try to put these rules upon a more satisfactory basis, they will advocate the compulsory

inspection of all boilers, whether made of iron or steel, during all stages of their construction, by thoroughly competent men, and will also use their influence to induce our legislators to make a law to that effect. The result of that would be this: Instead of plates being put into boilers with defects, cracks, &c., in them, which I believe is done, but not intentionally, the defective plates would be condemned by the inspector and the boiler made of sound plates; consequently, instead of the Board of Trade insisting upon all these percentages, increasing the thickness of plates to make up for the possibility of any defect existing in the plate, as pointed out and illustrated on the blackboard by Mr. Macfarlane Gray—but which perhaps is not there—they would dispense at least with Rules E, J, W, X, and Y. Most of us know that cracks in steel plates cannot always be seen at a glance; they must be looked for, and that very minutely. A part of my duties consists in inspecting steel boilers that have to bear working pressures varying from 120 pounds to 150 pounds per square inch, and I find that unless a careful inspection is made of plates from rivet hole to rivet hole before the boilers are put together, you cannot in many instances discover the cracks that exist. It will not do for an inspector to wait till a boiler is riveted together and ready for the water-pressure test, as many dangerous cracks may not then be discovered, as they may be hidden under laps, or, if exposed, be so fine as to escape detection, and the boiler would be passed as being perfect. The water-pressure test may not reveal the cracks, as they do not always weep, although you may have a test pressure of double the working pressure, even 300 pounds per square inch; but a competent man inspecting the boiler during its construction would object to plates being used that were in any way defective. I just throw out this hint because I think many people have an idea that all cracks in steel plates are so marked that you can put your hand in them, or, at least, they can be seen at some considerable distance; whereas, I find that, as a rule, they are simply very fine cracks, and necessitate a most careful examination, and perhaps ten to twenty times as long to inspect the boiler during its construction and water-pressure test as it would under an ordinary or casual examination, as adopted in many cases at present. Thorough and constant inspection during construction is, in my opinion, the only way of insuring reliable work on boilers, and the only satisfactory plan of reconciling the differences that exist between the Board of Trade and Lloyd's Registry, as set forth in this paper.

Mr. E. A. COWPER. I think we ought to be grateful to Mr. Milton for bringing this very important subject before us, as it is of very great importance to the mercantile marine that marine boilers should be reasonably constructed. I cannot agree with Mr. Macfarlane Gray that the evils of unequal expansion by heat can be overcome by great thickness of metal. The power of expansion by heat is only limited by the ultimate strength of the metal, and by increasing the thickness

and increasing the power the evil effects are in no way done away with, but rather aggravated. When heating up a cold boiler it is best to blow steam through a moderately sized pipe down into the bottom of the cold boiler full of water, and thus cause a circulation and an even distribution of heat throughout. There have been many cases of double-flued Cornish and Lancashire boilers leaking through expansion of the flues by heat, whilst the bottom and shell were comparatively cold. I have heard of cases where you could bear your hand on the bottom when steam was up in the boiler. Galloway tubes are very useful in helping circulation. With very long, egg-ended boilers, such as are used at iron-works, heated from below, the ends curve up considerably on being heated. The Board of Trade Rules made boilers heavier than necessary—I mean unnecessarily strong, particularly for high pressures, and the assumption that the area of the rivets and plates should be equal is not exact, as has been proved by the "research committee" of the Institution of Mechanical Engineers. Thick plates, say 1¼ inches thick, did not give as good a connection by riveting as when thinner plates were riveted together. Nothing but experiment would give the real strength of thick plates riveted together, or the right proportionate size and pitch of the rivets, for a given thickness of plate; this had been thoroughly proved by the experiments of the research committee, and they had obtained something like 7 per cent. increase of strength. Plates should be proved, and not "taken as of a certain strength," so that a boiler made of superior plates might be made lighter, just as mild steel boilers of proved plates might be made much lighter than if made with ordinary iron plates. It had been said in this room that the Board of Trade Rules were to be taken as correct with the "*usual proportions*" of rivets and pitch, but we were not told what were considered to be the "*usual proportions.*" Now, the "research committee" took a great deal of trouble to get at the practice of boiler-makers, and I am sorry to say that it varied so considerably that some were far wrong one way and some another, and there were only a very few near the best proportions of size of rivet to thickness of plate, and pitch of rivets for same. I have such a decided objection to very thick plates that I would never willingly use a 1¼ plate if I could possibly help it. If boilers were proved to double their working pressures, which was a rule I have settled for myself and always followed, they ought to be well within the "elastic limit" when so proved, so as not to go out of shape or strain at that proof, and there was not much margin wanted beyond that, in addition to something to allow for corrosion. Some time since, through the kindness of Mr. J. T. Smith, of Barrow, I had the opportunity of having a small boiler made of mild steel, ¼ of an inch thick, and 4 feet diameter, and having it proved, a lot of men at the pumps pumping up the pressure as fast as possible, when it began to leak at the longitudinal seams and rivets at 420 pounds per square inch, but it was impossible to burst it or rend it; it was afterwards

caulked up, and stood 350 pounds pressure. This occurred when I was in the chair of the Mechanical Engineers, and it was the more interesting from the fact of certain steel plates in boilers elsewhere giving way between the time of the making and the proving of this little boiler. Such experiments as this gave actual knowledge of facts, and great confidence in the admirable metal we now had at command. In reference to the amendments necessary in the Board of Trade Rules, it is, of course, only a question of time as to when they could be altered, for a wrong thing could not stand for very long in the face of the wide-spread intelligence of modern engineering; and it depends entirely upon ourselves to move in this matter, and lay before those in authority the very obvious improvements which might be made, and for want of which a trammel, or hindrance, is at present put upon real progress in marine engineering economy.

Mr. William Parker. My lord, and gentlemen, I was very pleased to hear our friend, Mr. Macfarlane Gray, enter into this discussion, and refer to the boiler which I had the pleasure of inspecting with him. Mr. Gray considers that Mr. Milton's statement that the circumferential seams of a boiler have little or nothing to do with the strength of the boiler is wrong. On that point I differ from Mr. Gray. He endeavors to prove his view by repeating that the boiler of the Thames, when getting up steam at a pressure of only 17 pounds, cracked. Well, that is not the only boiler that has cracked in the same way. I think all engineers here will understand and agree with me that if a stupid engineer in charge of a boiler raises steam very rapidly, the top of the boiler must become very much hotter than the bottom; you must naturally expect a great strain, even to the extent of rupture in the bottom plate. There is another case which I now remember, and which very likely my friend Mr. Laird will also remember, where boilers were strained in this manner to the point of rupture; that was the case of a large steamer at Liverpool with boilers of great length, some 18 or 19 feet long. When the steam was being raised in these boilers, the shell plate itself—not the seam, but behind the seam—ripped round for a distance of about 10 or 12 feet. Now, this crack was patched, and rigidly patched, and, to the surprise of everyone, the plate on the opposite side gave way in a like manner. I think these boilers were patched on three occasions, when, from a happy thought of some one or another in connection with Mr. Laird's firm, it was suggested that a patch should be put upon each crack, having slightly oval holes, so that when the upper part expanded the lower part would yield to this excessive and irresistible stress. That was done, and the vessel has been sailing without trouble ever since, I think for a period of about ten years. Now, to my mind, that proves that any amount of metal that might have been added would not have overcome this irresistible strain, caused by unequal temperatures, and I think the argument Mr. Macfarlane Gray puts forward altogether falls to the ground. However, the discussion, I fear, has somewhat drifted into a discussion

on boiler-making generally, and I would like to lay before this meeting simply and plainly the difference that exists between the rules of the Board of Trade and those of Lloyd's Register for fixing the safe working pressure on the shells of cylindrical boilers. I have on one occasion before at these meetings had to refer to this matter, and if I use the same argument again, I hope you will excuse me. It is, I think, admitted that if a decrease of consumption, or more economical steamships are to be obtained, pressures must rise. When mild steel was first introduced for marine boilers—now some five or six years ago—I had the honor to read a paper here on that subject. At that time we had had only one or two boilers made of steel, and the average pressures were then under 75 pounds. Owing to the introduction of steel and other improvements they have since risen, as Mr. MacColl has told us to-day, to 90 pounds, and I take it that steamship owners would not go on adding weight and increasing pressures unless they obtained an equivalent reduction in fuel. At the present time, owing to the operation of the rules of the Board of Trade with regard to the shells of these boilers, it is not possible for higher pressures than 90 pounds to be carried in passenger steamers, unless enormously thick shell plates be used, or the boilers made so small and so numerous that the freight lost by the extra space taken up by the boilers is greater than the money saved by the increase of pressure. Last year we had a paper read upon a steamer with boilers constructed to work at 125 pounds pressure; that vessel has successfully traveled 43,000 miles without any hitch whatever, and with a considerable saving of fuel. According to the rules of the Board of Trade, the boilers of this vessel would not be eligible for more than 93 pounds pressure, but they have been proved by actual work to be quite efficient and perfectly safe at a pressure of 125 pounds. If the Board of Trade Rules were altered to admit of this pressure being carried in boilers of large dimensions, then steamship owners, whose trade compels them to obtain passenger certificates from the Board of Trade, would, like the owner of that vessel, be able to save considerably in the consumption of fuel. The point at issue is of the simplest character. We are now using steel. We test every plate, and we have drawn so many seams asunder, as Mr. Cowper has told us, that we now know pretty well the percentage of strength of any form of joint as compared with the strength of the solid plate. I take it, then, that there is not a very large factor of safety required for either possible inequalities in the strength of the material or uncertainty as to the strength of the joint. Again, we know that the strength of a cylinder varies inversely as its diameter in inches; it is not as if it were a rectangular boiler, or an old wagon-shaped boiler, such as we had years ago, and for which we required a high factor of safety; but here we have a structure with regard to which we know how its strength varies, and I submit that to cover any other contingencies, we do not require such large factors of safety for thick plates as the Board of Trade insist upon. The only other thing for which a factor of safety

is necessary is corrosion. Now, it is quite clear that the amount of corrosion is the same in a thick plate as in a thin one. We will just take the case of two boilers, one with shell plates $\frac{3}{8}$ of an inch, and the other with shell plates of 1 inch thick, and assume that the corrosion is an eighth of an inch in each case, then it follows the $\frac{3}{8}$-inch boiler will be reduced one-third, while the inch boiler will be reduced only one-eighth. Now, the difference between the Board of Trade Rules and those of Lloyd's Register is this: we give to the thick boiler at the beginning of its life a larger pressure, or, in other words, approve of a less factor of safety than to the thin boiler, so that at the end of the lifetime of both boilers the thick boiler may have the same factor of safety as the thin one. I think that is all I have to say, except that I hope the reading of this paper and the discussion which has followed upon it may do something towards bringing about further improvements in marine engineering consistently with perfect safety.

Sir FREDERICK BRAMWELL. My lord, I should like to say a few words upon this paper. The question of an official inspection of boilers is, of course, a very wide one. It involves considerations of principle and considerations of detail. I presume at the present time the public would not be content that there should be no official inspection of marine boilers, although, fortunately, there is not one of land boilers. I say fortunately, and do so for the reasons I gave about twelve years ago, when, as I think, a very unwise attempt was being made to put every boiler of every kind under an inspection which, although it was called an inspection by a steam-users' association, would in truth have ended in an official inspection. I think it was about twelve years ago that this was attempted, and the mechanical engineers of London, some twenty of them, got together, and gave such evidence before the committee as to put a stop to that which, in my opinion, would have been most mischievous. I know it is very easy, when a man cannot disprove a statement in any way, for him to say, "It is based on figures;" then there is a laugh, and the thing passes over; but I brought before the committee then a statement which I think had some weight with them, and that was this: I showed them there that the Rules would apply to boilers working that most wasteful kind of engine used on land the low-pressure non-condensing engine, and that the result would inevitably be that the inspector in the district, as he would be desirous, of course, of having no explosions in that district, with the object of preventing that, would insist that many of the boilders should be discarded or their working pressure lowered. Owners, if of not much capital, would not like to discard their boilers, and therefore would lower the pressure. A computation was made, to show that probably the result would be to increase the consumption of coal throughout England by four million tons a year. And then this was put to the committee: the statistics of coal mining prove that, taking one year with another over a lengthened pe-

riod, there is in round numbers one fatal accident for every 100,000 tons of coal raised. The result, therefore, of an increased consumption of four million tons per annum would have been not only a waste of four million tons of our store of coal, but would have been the loss of forty lives of persons engaged in coal mining. The statistics brought forward by those who were advocating the inspection showed that the total loss of lives from all explosions of boilers was, I think, eighty. When we came to analyze that, it turned out that there were a certain number of these due to the bursting of kitchen-range boilers and of boilers of that kind, which, of course, could not be touched by any inspection. There were a great number also due to avowed negligence by attendants on boilers, which could not be touched by inspection of the boiler itself; and when such explosions as these were eliminated from the total number—I am speaking now from memory, but my impression is—it brought down the number of deaths due to explosions from preventable causes, and that might possibly have been avoided by care in the making of boilers, to something like fifty-five. It further appeared that the result of inspection was by no means the obtaining absolute immunity from accident, and that thus only forty-six lives instead of the fifty-five would be saved under universal compulsory inspection, while the increased consumption of coal would have cost a loss of the lives of forty miners. Under these circumstances, I ventured to tell the committee that anything might be bought too dear, that you might buy gold too dear, and I said you might buy human life too dear when you bought it with other human life. I am glad to say the committee rejected the attempts of those who were seeking to force this upon us and left the matter where it was before, viz, that a man should suffer for his own misconduct, if he was guilty of misconduct, in the construction of boilers. That which I have been saying is somewhat, I must confess, beside the particular question before the institution, although it touches upon the general question. But it does touch even upon the particular question in this way: the inevitable tendency of any department which has responsibility cast upon it is to take care that whatever happens there shall be no discredit cast upon that department, and this drives them to overcaution, involving needless expenditure, and rendering progress and improvement difficult. I remember at that time it was asked of me, in cross-examination by certain members of the committee, "What do you say to the exercise of Board of Trade influence upon marine engineering?" I said that if it had not been for the exercise of Board of Trade influence upon marine engineering, I believed marine engineering would have improved very much more than it had up to that time. At the date of this committee, marine engines were working at a pressure of about 30 pounds to the inch. Turning to the Board of Trade Rules themselves, it has been said, and with great force, as it seems to me, that these err on the side

of excessive precaution. They begin with a factor of safety of 5; I always thought that the factor of safety was for the purpose of making allowance for those unforeseen matters which might render your boiler less safe than you thought it was. But when one reads through these regulations, one finds that every attempt is made to get rid of all these chances of uncertainty. For instance, in the factor of safety of 5, nothing is allowed for inferior plate, for although a by no means high quality is assumed in the formula, yet if the inspector is not satisfied, if he at all doubts the material, there is an addition to the divisor: "X. ·4 to be added when the iron is in any way doubtful and the surveyor is not satisfied that it is of the best quality." That is an addition to the factor, taking, therefore, about one-twelfth off the pressure. Then, if the boiler has not been open to inspection during the whole period of its construction, 1·65 is added. Now, under this head, you absolutely eliminate all doubt as to the character and quality of the manufacture, because it has been open to inspection the whole while; you have previously eliminated all doubt as to quality of the iron, and having done that, you still have a factor of safety of 5, whereas if you do not eliminate those two things, you are to have a factor of 7·05. Well, is that really reasonable? Where everything is known, is not all that is really wanted a fair allowance for possible wear before a resurvey, when the pressure to be carried may be readjusted, and the certainty that the boiler will not be strained beyond its elastic limits. Now, although the whole of these rules are directed to getting rid of anything in the nature of uncertainty, there is still imposed as a minimum a factor of safety of 5. I cannot help thinking that that is not a reasonable condition, and I do believe that it presses very heavily upon improvements in marine engineering, and upon our power of competing with other nations who are not weighed down to the extent that we are. Every one must agree that the officers of the Board of Trade desire most rightly and honestly to do the duty cast upon them, and I hope to be understood as only calling attention to the fact that when a man is in a position of responsibility in respect of boilers, the first thing that man takes care of (and it is only human nature), "Whatever else happen, I must be able to report that no explosions have taken place in any boiler I have surveyed or passed."*

*[The following letter has since been received from Sir Frederick Bramwell.—Ed.]

37 Great George Street, Westminster, S. W.,
March 22, 1883.

Dear Sir: I had no sooner left the meeting, than I found, to my regret, I had omitted a very striking illustration of the overstringency of the Board of Trade Rules, viz, that if they were applied to the boilers of locomotives, they would reduce the ordinary working pressure of 140 pounds to so low a point that the whole railway service, both passenger and goods, would be paralyzed. It is some time since I made the calculation, but my recollection is, that the application of the Board of Trade Rules to a new boiler, designed by one of our best locomotive engineers, would have resulted in a working pressure of only 80 pounds instead 140 pounds. In the interests

Mr. MACFARLANE GRAY. My lord, may I say one word of explanation? Dr. Siemens, and Mr. Parker also, attempted to wipe out my remarks about the ripping of a transverse seam. They seem to have misunderstood me, and to think that I recommended (although it was not I at all) thicker plates to prevent the seam ripping. But what I said was this: Granting that that would not prevent the seam ripping, when the seam has ripped what is keeping the boiler together? It is the remaining seam that is keeping the boiler together, and if additional strength is not in it—not in the piece that is ripped, of course that is of no value; but when that plate is ripped it is the remaining part, and if that is not of unusual strength it cannot stand the increased pull that comes upon it.

Mr. J. WRIGHT. My lord, I would like to trouble you with a very few remarks with reference to one part of the subject under discussion, viz, the strength of circular boiler shells and the factor of safety. I think that hitherto we have been over-cautious in this matter, and have looked too much at the wrong end of the subject. Mr. Parker has indicated in his remarks the view which is taken on the subject in carrying out the practice at Lloyd's, and this certainly appears the reasonable view to take. What we should determine is what would be a reasonable factor of safety, or margin of safety, when a boiler would be considered worn out, and then add to this a proper amount for wear and tear, having regard to the conditions of service and the time the boiler would be expected to last. A fixed factor of safety for a new boiler shell may be unobjectionable for a definite pressure or for pressures not differing much, but it is certainly unnecessary to apply the factor of safety which was used for the steam pressure of a few years ago, say 75 pounds, to the pressures now in use and coming into use, say 100 to 150 pounds, while the conditions of wear and tear, &c., would be very little altered. The *margin* of safety is really what should be taken for guidance, but this, with the usual factor of safety, becomes very much more than appears to be necessary under any conditions at the higher pressures. I may mention that the practice carried out at the Admiralty for some time has been, in the main, on the same lines as Lloyd's Rules.

Sir EDWARD REED. May I make an observation or two? I rose at the same moment with Mr. Wright to make the remark that he has made, and therefore I will not repeat that; but I have watched this paper and the discussion rather from a parliamentary point of view, because it seemed to me that if there is solid justification for the very

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of speed, and of the power of making full use of our railways and of their plant, and in the interests of economy of fuel, let us hope that the locomotive engineer may forever be free from Board of Trade trammels.

Believe me to be yours, very truly,

FREDERICK BRAMWELL.

GEORGE HOLMES, Esq.,

Secretary Institution of Naval Architects, 5 *Adelphi Terrace, W. C.*

broad statements made in this paper as to the effects of Board of Trade inspection, Parliament is the only place where effect could be given to the opinions which have been brought forward, and I had some idea of putting a question or two in the House of Commons to the president of the Board of Trade upon this subject. The effect the whole discussion has had upon my mind is this: That the Board of Trade Rules, however meritorious they may have been in the past, are operating perniciously now in two respects, both of which have been spoken to. One has been spoken to by Dr. Siemens and the other by Sir Frederick Bramwell. The first is this: They seem to me to operate against that most important feature in the progress of boiler construction, as in all other constructions in metals, that of thinning the material in proportion as you improve its quality. Mr. MacColl interpreted Dr. Siemens as saying that he would thin the plates almost indefinitely, but I do not understand that to be Dr. Siemens's idea. If I understand him aright, he treats a boiler as a water-holder for the mere purpose of keeping the water together while it receives the application of heat from the outside, and it requires to be thick enough to give the strength necessary for that purpose, and no thicker, except for the sake of the factor o safety. I do believe myself, from my limited experience in boilers, that Dr. Siemens is not only right, but emphatically right, and that a very large part of the mischief that there has been in boilers in the past has arisen from the fact that through the very inferiority of material we have been driven to thick plates, which are attended by the disadvantage that Mr. Cowper has pointed out, and many other disadvantages which we are all familiar with. The use of thick plates in boilers has, therefore, been at once a consequence and a cause of mischief. Therefore, it does appear to me that the present system operates either against the introduction of new and improved material, or else against getting the benefit of the improved material when you have introduced it. If that be so, it seems to me it is a very serious question. Then, with regard to the other question, the factor of safety. These rules do seem to me to operate injuriously. I would illustrate that by reference to another fact, which will be familiar to the minds of all those here, that while we are imposing through the action of the state a factor of 5 (increased, as we have seen, in various ways) upon the boiler, we are content with a factor, if I remember rightly, of less than 2 in the case of the capability of the ship herself to withstand the strain thrown upon her in a sea-way. You will remember, no doubt, that Mr. John read a paper at this institution in which he showed the result of calculations made upon the strengths of ships to withstand strains at sea, and he there brought out what was to my mind a very startling fact, although I was not wholly unprepared for it, viz, that the factor of safety of a ship as regarded the strain in the sea-way, and therefore as regarded the capability of the bow of the ship to carry the boiler at sea, was really extremely low. I am not quite sure whether it was not less than 2. If that be so, to

go on imposing a factor of 5 upon a new boiler made of such admirable steel as is now produced does really seem to be improper. I am sorry to trouble you with this, but I should like to act wisely in the matter, and if it happen that we are right in supposing that the Board of Trade Rules in maintaining an unnecessarily high factor operate against the introduction of improved material, which is so advantageous in many ways, some steps ought to be taken to get them amended.

Mr. MILTON. My lord and gentlemen, I will not take up more time than is necessary in addressing the meeting, but I think I must make a few remarks upon those made by Mr. Macfarlane Gray. I clearly state in this paper that it is the strength of the boiler to resist internal pressure for which we take the proportions of the longitudinal seams, and that it is with regard to the internal pressure that we may ignore the circumferential seams. Now, Mr. Macfarlane Gray stated very clearly that the result of all experience of engineers is that the circumferential seams are the seams which give trouble in boilers. This is perfectly true. As a particular case he mentioned that of the Thames, where the circumferential seams failed; but these seams did not fail through the pressure of the steam; they failed through the differences of temperature and the consequent differences of expansions on the different parts of the boiler. It is this which makes the circumferential seams leak and give trouble, and in some cases breaks them; but to provide against this you must look to the design of the boiler itself. By putting on factors of safety you make plates thicker, but you do not alter the temperature in the least. You increase the strains in precisely the same proportion as you increase the thickness of the plate to withstand those strains, so that the material is just as much strained with thick as with thin plates.

Mr. MACFARLANE GRAY. Will you please answer my point of explanation given just now?

Mr. MILTON. The design of these circumferential seams does not influence the safety of the boiler at all; the pressure on the ends after the plate is torn is still taken by the stays, which are there to take the pressure before the plate is torn. And if, after the seam is torn, you can only keep it tight it will never open any further. That has been shown by the boilers that Mr. Parker has referred to. I think the case he must have had in his mind was that of the steamer Egypt, which, I think, had several of her boilers torn in the bottom. They were patched at last with a flexible joint; that is to say, with a joint which gave no longitudinal strength whatever, relying entirely upon the strength of the stays to take up the longitudinal pressure, and those boilers have done good work ever since.

Mr. MACFARLANE GRAY. They were not cracked. I had the supposed cracks chipped across, and the chipping came off solid with the "crack" mark on it; it was a mistake supposing they were cracked.

Their own inspector could not get in. I got in under the furnaces and had this done.

Mr. MILTON. At any rate, in the case of the Thames the plates were cracked. I will mention the case of a steamer belonging to the General Steam Navigation Company. About seven years ago, I saw her boiler, of which the seam was torn for about ten feet—nearly a third round the boiler. That boiler afterwards steamed round to the north with an opening in the plate an eighth of an inch wide made tight by a covering patch, but there was no longitudinal strength added. Seeing that the safety of the boiler is not influenced in any way by the circumferential seams, and that the design of these seams affects only the lifetime of the boiler, or the trouble that will be occasioned in keeping the boiler tight, it should be left entirely to the owners to say whether they will have badly proportioned or well proportioned seams; they will soon find out what are the best. As a matter of fact, all circumferential seams in double-ended boilers are made with double-riveted lap joints, not with double-riveted and double butt-strap joints, to which the Board of Trade allow the smallest factor of safety. After all, the efficiency of these circumferential seams must depend upon the proportion of the diameter and the pitch of the rivets to the thickness of the plate, and that Mr. Macfarlane Gray says must be left to the ordinary practice, which Mr. Cowper has shown varies very much in itself. With regard to the statement of Mr. Macfarlane Gray, that the constants given in the Board of Trade Rules are not accumulative, I have here a paper containing particulars of boilers being made by one of the most eminent engineers in the country, under Lloyd's Rules, for a pressure of 140 pounds. This case will also, in some measure, answer the point raised by Mr. MacColl as to the amount of difference between Lloyd's and the Board of Trade Rules in steel boilers with thick plates. The boilers are passed by Lloyd's Register for 140 pounds. I have seen the tracings of the boilers myself. I am sure they are entirely in accordance with Lloyd's Rules for that pressure. They were submitted to the Board of Trade, who say that 107½ pounds is the working pressure according to their rules. This is obtained by adding in the following amounts to the factor 5, for the perfect boiler. First, they add .3 under clause B, that is, for drilling the holes in the longitudinal seams before the plate is bent; then .15 under clause G, for drilling holes in the circumferential seams before the plate is bent. Next we have .1, in accordance with clause S, for the double-riveted lap joint in the circumferential seams; and, finally, .3 for the double-ended boiler, as required by the provision V. I may say that these boilers are made with 1⅛ plates to meet our rules for steel boilers, and I think that is nearly as thick a plate as can well be made of steel. I would not like myself to use thicker plates. If these boilers had to pass the Board of Trade Rules for 140 pounds, the plates would have to be 1 inch and 7½ sixteenths. Mr. MacColl has said that corrosion has now been mastered.

Well, it may have been mastered by a few people, but it is not generally mastered. Corrosion, unfortunately, still takes places in very many boilers, and this must be expected when we consider that boilers are sent to sea and are put in charge of men who perhaps have never seen the ship until a few days before she starts—that is an ordinary circumstance of the life of a boiler—and that the engineers are changed continuously. Even if corrosion is mastered by a few engineers, we cannot expect that the majority of sea-going engineers have mastered such a difficult subject—a subject studied for many years by the boiler committee, and even now not completely mastered by them. Mr. MacColl mentioned also that the Board of Trade give greater pressure for flat plates of steel than for those of iron. When the first steel boilers were made, and they were approved of by Lloyd's Register, it was thought then that steel being a stronger material than iron as regards tension, a smaller thickness of plate could be with safety applied. Several experiments, however, were made (some of them by Mr. Marshall, of Newcastle), which showed that steel was very little, if at all, superior to iron for these purposes in point of strength, especially when you came down to the very mild steel, which is the best for the flat plates of boilers. Lloyd's, in consequence, now give no reduction whatever for flat steel plates for boilers as compared with iron. This, however, has never worked disadvantageously to the boilers, for the flat plates are not the obstacles to increased pressures. I have nothing more to say, except to thank the meeting for the way in which they have received the paper.

The PRESIDENT. Gentlemen, before I convey, as I am sure I shall be allowed to do, your thanks to Mr. Milton, perhaps you will permit me to say a very few words. You can readily believe, gentlemen, that this is not the first time that this question of vital importance to the mercantile marine, as I believe it to be, has come before my notice. We heard a great deal about it before the commission on unseaworthy ships, and the great complaint then was that ship-owners and ship-builders were expected to comply with rules about which they knew nothing, and which they never saw until the ship was either accepted or condemned. That has been corrected; it was a very just complaint. It was the non-circulation of the Board of Trade Rules that was the great cause of complaint. That has been corrected. That shows at least that the Board of Trade is open to conviction. Now let me say one word on behalf of the Board of Trade, because I like to take both sides of the question. We must remember the tremendous responsibility that rests upon the Board of Trade with regard to the thousands of lives of emigrants and other passengers which are committed to their charge annually. The country looks to them to protect those lives, and, therefore, if they err on the side of caution, I am sure you will admit they are rather erring upon the right side than upon the wrong side. I say that for the Board of Trade, and on their behalf. Now we

come to another complaint, which is clearly a general complaint, on behalf of the mercantile marine, that the rules of the Board of Trade with regard to boiler plates err in excess of thickness. That point has been put home by Dr. Siemens, by Mr. Cowper, the ex-president of the mechanical engineers, by Sir Frederick Bramwell, whose reputation is world-wide, and lastly, but certainly not least, by Mr. Parker, who was himself an officer of the Board of Trade, and who is now the chief engineer of Lloyd's. No public department can by possibility afford to neglect such voices as those. I have shown you that they are open to conviction, and when you find between the two controllers of the British Mercantile Navy, viz, the Board of Trade on the one side and Lloyd's on the other—because the term is not too strong—this immense divergence in their rules in regard to the construction of boilers, I say it is time at least that some attempt at reconciliation between these rules should be made. And I am sure I know enough of the Board of Trade, and I know them pretty nearly as well as anyone here except the people inside it, to be certain that they cannot afford to neglect the subject-matter of this discussion, and therefore following up my friend, Sir Edward Reed, in Parliament, I would make a very humble suggestion; take it for what it is worth, but I am an old Parliamentary practitioner. I would suggest that we invite the Board of Trade to a conference upon this subject. I think that if a powerful deputation—not too large—of our most eminent engineering members requested an interview with the Board of Trade, it would have a very good practical effect. If you think well of that, I, as your president, not as myself, would be most happy to undertake to make the ordinary arrangements. It must be something more than an ordinary deputation; it must be a conference, because this question affects the whole of the mercantile marine of this country, and therefore it is impossible to overestimate its importance. I hope you will forgive these few observations; but as they are based, to a certain extent, upon Parliamentary procedure and experience, I venture to make them to you. If you agree to them, I will undertake to arrange such a conference, and I am sure the Board of Trade cannot resist the application. They cannot; they would not wish to do so. Now, gentlemen, I am sure you will allow me to convey our united thanks to Mr. Milton for bringing before us this, which, I venture to say, whatever important points we may have brought before us, is as important as any question that can be raised within these walls.

VI.

ON CERTAIN POINTS OF IMPORTANCE IN THE CONSTRUCTION OF SHIPS OF WAR.

By Capt. G. H. NOEL, R. N., *Associate.*

[Read at the twenty-fourth session of the Institution of Naval Architects, 14th March, 1883; the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

I should not have intruded upon your valuable time had I not felt that in this institution—which comprises among its members and associates not only the most eminent experts in the great engineering science of ship-building, but also the merest amateurs in the art—the opinion of naval men may be useful, and even valuable, since their duty is to command, and if necessary to fight, those vessels which the naval architect designs and constructs. Being in the position of an amateur, who has carefully watched and endeavored to think out many of the questions relative to the construction of ships of war from a practical point of view, I have undertaken to read this paper, and submit it for discussion at this meeting.

The number of interesting and important considerations embodied in the science of ship-building is so great and varied that it would be far beyond my power, even if time allowed, to touch on more than a very few of them. I therefore propose to confine myself to certain points, the great importance of which may possibly in some cases be underestimated. They are:

I. On the strength and height of the bow necessary for ramming.
II. On water-tight compartments.
III. On armored conning towers.
IV. On torpedo defense.

I.—*On the strength and height of the bow necessary for ramming.*

In taking his ship into action it is most desirable that a captain in Her Majesty's service be fully confident the ram with which she is provided is of such construction that he need not fear the consequences of charging the enemy, should he have a fair opportunity of doing so. Without that confidence he would fight but a half-hearted battle; and considering the enormous weight of responsibility that rests on him, it is most essential that he should be able to fully rely upon the power and efficiency of his vessel, and of the weapons on which the issue of the fight must so greatly depend.

The late Mr. Scott Russell—a much-lamented friend and supporter of this institution, whose memory is pleasantly associated with it by the thankworthy consideration of the council in giving us an admirable portrait of him in the last number of our transactions—in a paper read at the Royal United Service Institution, June 8, 1877, speaking on this subject, says: "What we want in collision construction is that the strong thick parts of the ship should grow evenly out of the weaker thinner parts abaft, so that the strain of collision shall be spread over a wide space, thus utilizing the strength of a large extent of good iron. For this purpose the stem must be formed by gradually growing out of the ship's bow, in plates overlapping thicker and thicker, until they become a thick, smooth, inflexible stem. The same principle must prevail throughout the whole bow of the ship. The decks must all be incorporated in the interior of the hull with the skin and the stem, so as to form an entire hollow weapon of great strength and homogeneous structure." These words should still have their full significance in guiding present and future construction; not that there is any want of care on this point, but because in the ever-increasing number of subjects to be considered, all involving new difficulties and complications, the obstacles in the way of providing the necessary strength for the ram increase, and the extreme importance of doing so is apt to be undervalued.

The nature of the strains brought on a bow when ramming is patent to all here. The dangers which would arise from imperfect construction or design may be classed as follows:

1. Of the ram bow being actually forced in.

2. Of the bow being twisted and the stem broken.

3. Of the vessel being herself sunk, which might result from two causes, either the want of height in her bow and freeboard, causing her to lurch over and so capsize, or to go down head-first, or else, owing to weakness in the bow upper works, which, breaking away, might lock with those of the enemy's ship, and so cause both vessels to sink together.

With reference to the first danger, there is no difficulty to be apprehended in obtaining the desired strength in the bows of comparatively small vessels, so long as wrought-iron or steel stems can be used; but with increase of size and weight we get an increase in the momentum, and in some cases a reduction in the cushioning effect, which so much lessens the enormous strains consequent on ramming. For these reasons great additional strength is required in heavy vessels. It is questionable whether this is fully appreciated in the construction of citadel ships, where the stem—though to a small extent backed up by an armored deck—is unsupported by side armor, and the principal weights are concentrated in the center of the vessel some distance from the bow, upon which the whole effort of bringing this mass to rest devolves when the ramming is direct. In some of the armored and other cruisers the sys-

tem of coppering the bottom over wood sheathing has been adopted, necessitating the introduction of gun-metal stems. The prudence of this measure may also be questioned, as it is doubtful whether the necessary strength to withstand the shock of ramming can possibly be obtained with brass, even by using an additional weight of metal with the intention of supplying this deficiency. It would certainly be advantageous if some means could be discovered whereby a less brittle substance than gun-metal could be employed for the stems of these vessels.

Secondly, as regards the twisting of the bow and breaking of the stem. Examples of this danger are continually brought to our notice by collisions at sea, notably on the occasion of that disastrous accident which happened to two German iron-clads in the Channel a few years ago. The constructors of English ships of war have wisely curtailed the length of the ram-bows of our ships, a measure calculated to give them greater power to resist oblique or twisting strains; but is it not desirable in our heavier iron-clads that a more secure *root* be constructed to the ram? This might be accomplished by building its point on to a cigar-end-shaped structure worked smoothly into the bow, and giving the sharpness requisite for the speed of the ship by its horizontal as well as its vertical entrance; in fact, like the lower part of the stem of the Polyphemus, without its torpedo discharge-pipe.

Thirdly, as to the height of the bow required for efficiency as a ram, and the strength of the bow upper works. The first part of this question applies to coast service iron-clads with low freeboard, denominated "rams," and which are often spoken of as being more serviceable in that capacity than larger and more unwieldy ships. Some of these "rams" are constructed with fairly high bows, and when fighting in smooth water would be most formidable; but no vessel of this type can give her captain the confidence he would possess were he commanding a ship with a high, well-constructed bow, which would insure the enemy he had rammed being thrown off clear, and at the same time would cause him no misgivings as to the result of entanglement with his foe, or of a heavy and severe lurch after drawing clear of her. The strength of the upper works of the bows of high freeboard ships is undoubtedly sufficient in all cases except those of vessels constructed of light steel, where the danger may imperceptibly creep in of making the upper works forward too light to be able to resist the pressure brought on them by ramming—a weakness which, if not provided against, may lead to disaster.

Ramming is a science which cannot be practiced; we are therefore dependent on accidental collisions—unhappily only too numerous—and a few cases of ramming in action, for information as to its effect. With a fleet such as that possessed by England, the tactics of ramming must ever be considered of very great importance. In using these tactics every endeavor should be made to inflict the most fatal blow on your

enemy with the least damage to yourself—that is, to ram scientifically. But since it is impossible to practically study the art, there is no doubt that in a ramming combat the shocks and strains experienced will be much more severe than would be the case were such practice possible. It is only natural, therefore, that the strength and efficiency of the bow for ramming should be deemed by naval officers a matter for most urgent consideration. This is one of the points I wish particularly to impress on the meeting.

II.—*On water-tight compartments.*

Water-tight compartments in a ship of war are required to prevent her from sinking after being severely wounded in battle, as well as, in common with other ships, when harmed by collision or grounding. The principal object is, therefore, to design effective water-tight compartments, the bulkheads or vertical sides of which can be closed in action without impairing the necessary communications, and without detracting from the draft and ventilation requisite for steaming at full power.

Were it possible to abolish all such dangerous appliances as water-tight doors the safety of ships would be vastly increased. This is of course impossible in central citadel ships constructed with armored decks before and abaft the citadel a few feet below water, and may be considered to detract in some degree from the value of this type of ship. In belted iron-clads there is no reason why the only communication with the principal compartments should not be from above, for where there is a convenient deck fore and aft above the water-line, the communications with and stowage of these compartments must be a simple matter enough. All pumping and ventilating pipes, drains, and other breaks through a bulkhead are sources of weakness, and however well fitted with valves and cocks (whether automatic or worked by hand), are liable to be found faulty when the crucial test is applied. Compartments with intact bulkheads, communicated with, ventilated and pumped out only from above, and having their deck or decks fitted with water-tight hatches, would insure the greatest possible degree of safety to the ships.

In armored vessels of the central citadel type, where it is imperative that the armored deck be intact, the only plan which seems to carry with it any degree of security is that of constructing before and abaft the citadel, "passages" or "shafts" in the center line of the ship immediately under the armored deck, having one end opening upwards into the inside of the citadel above the water-line, these passages extending as far as the last compartment at each extremity of the vessel, and being the only means of communication with the lower compartments aft and forward. The entrance to each separate compartment would have its water-tight door, and the pumping-out and ventilating pipes of each compartment would also be led directly into the passage. A shaft so placed would be in as secure a position as possible from the disruptive effects of a successful ram or torpedo attack. There might

be difficulty in the stowing of the compartments, but this plan would at least admit of all the bulkheads being intact, except where there are openings and pipes into the shaft, the doors and valves of which could be worked with a fair amount of security from the passage itself. Any compartment which could open directly into the citadel would have the same element of safety as those in belted ships, as before described.

One of the matters to be attended to in action is the safety of those men whose duties employ them in the lower compartments of the ship where torpedoes are worked, cartridges and projectiles are issued, and other business is necessary. An escape must be provided for them in case of their being flooded out, and such escape ought to be so arranged as to cause no great additional danger to the ship should a door or hatch be left open or unfastened in the hurry of their retreat. This escape would be perfectly provided for in belted ships where the compartments open only upwards, and would be fairly provided for in citadel ships fitted with central fore and aft shafts, as suggested.

Another source of danger which may appear in action, and which requires attention, is that of a panic in the stoke-hold. This may be said to apply principally, if not solely, to ships fitted for forced draught, where the stoke-hold is closed in air-tight, and the pressure in it raised above atmospheric pressure by means of fans. It should be considered essential in all ships that a means of escape be provided for the stokers up the funnel casing, or some convenient passage; if this is not done, the knowledge that they are hopelessly shut in may lead to a panic, and cause great confusion, if not actual disaster. One of the chief anxieties to a commanding officer in battle will be to keep the necessary command of speed on his ship, as the failure of the engines at a critical moment might be fatal. This failure could result from several causes, not the least serious of which would be a panic in the stoke-hold.

The labor and ingenuity which have been devoted to the construction of water-tight compartments in ships of war, and the admirable results obtained, are most fully recognized in the service. Our recent iron-clads—built with double bottoms, wing passages, and numerous internal compartments, all with well-strengthened bulkheads—are as safe as floating vessels can be; we may reckon on their withstanding the explosion of two, or perhaps, three Whitehead torpedoes under them, or even the shock of a ram, without being sunk. But there is this condition: *all door and valve connections between the compartments must be securely closed.* Even with this high degree of efficiency there still exist certain points, which, viewed from the position of a commanding officer about to take his ship into action or danger, may be considered wanting in perfection, and should, if possible, be rectified. Some of these points are the outcome of new designs; others are admitted weaknesses, which cannot be conveniently avoided; and a few result from overconfidence in mechanical contrivances, the practical working of which naval men are inclined to view with suspicion. In the navy

we are trained from boyhood to act upon two great maxims, namely, "To take every precaution against accident," and "To arrange everything that may have to be done on an emergency so simply that there can be no likelihood of mistake or confusion." Now that vessels of war are yearly becoming more complicated in their build, we are naturally anxious to impress naval architects with the importance of carrying out like principles in the construction of our ships, with the view of rendering them as easily secured from harm as possible, when dangers really occur.

III.—*On armored conning-towers.*

The conning-tower, or captain's turret, is the place from which the ship herself is managed, and from which the various weapons are directed. A ship without an efficient conning-tower may be compared to a man without an efficient head-piece; and yet, until quite recently, few matters affecting the fighting efficiency of the ship have had less attention paid to them.

Exposure to view, and, in some instances, the position of the weight to be carried, curtail the height of the conning-tower; but, apart from this, nothing should be allowed to prevent its being placed in the position best adapted for an all-round view, especially clear ahead, and at the same time favorable for conning and steering the ship, for directing the guns and torpedoes, and for communicating with the engine room, batteries, &c.

The following is a brief description of what may be considered an effective arrangement for conning in an armored vessel (see drawing). A bridge or platfrom across the ship, strongly constructed, and having a bullet-proof parapet or netting around it, about four feet high, which will also protect the occupants from the concussion of their own guns. In the center of the ship, and standing to a height of 5½ feet through the middle of this bridge, an armored oblong tower, reaching down into the ship below the level of her side armor, and having above the bridge four armored wings, which would provide shelter for the officers outside of the tower. It is seldom, if ever, possible to effectively con a ship when maneuvering with others from an inclosed tower, and it will be found even more impracticable to do so in a fleet action; the commanding officer will often find it necessary to station himself, and perhaps some of his assistants, on the bridge, and the protection of armored wings, such as those referred to, may well be afforded, under which these officers may take shelter when passing close to the enemy, if only to shield them from the effects of the concussion of the opponent's heavy ordinance, and the searching fire of machine-guns. The tower itself would be a structure of immense strength, and of sufficient size above the bridge to admit of room for freely working all the appliances necessary for steering the ship, directing the guns, and communicating with the different parts below; all such appliances being

arranged with the utmost care and precision. The armored wings on the bridge outside the tower would stand out from it at angles of 45° with the fore and aft line, those on the fore corners being about 3 feet in length, and those on the after corners 5 feet; this arrangement would least interfere with the "directors" for the guns. A bullet-proof roof or shield would cover the whole tower and extend over the wings, the edges capable of being raised on either side, in case of the ship getting a permanent list. For going into action, the bridge must be entirely clear (any temporary chart-house or other obstruction being removed), it would extend sufficiently far towards the ship's side to command from its ends an unbroken view ahead and astern, and from it the commanding officer should have full control of all that is going on inside the tower. Duplicates of the tell-tales, indicating the movements of the engines and rudder and the speed of the ship and engines (when these last are supplied), would be placed on the fore edge of the bridge,

ARMOURED CONNING TOWER.

Fig. 1.

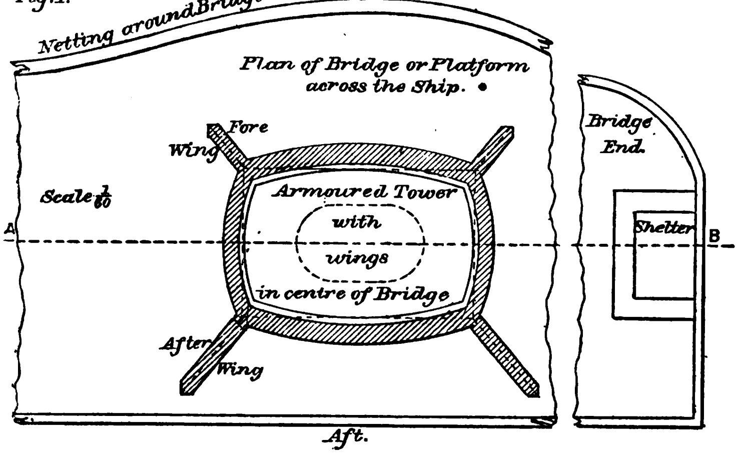
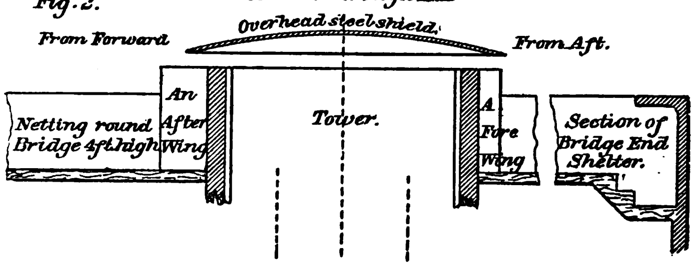


Fig. 2.



NOTE.—Width of bridge or platform will depend on the nature of the vessel; and the depth to which the armored tower will have to be taken, on the position of the side armor.

in conspicuous positions, and, lastly, bullet-proof shelters would be fitted at the bridge ends, which, however, must not show above the bridge netting so as to obstruct the view from the directors.

The essentials for efficient conning and directing arrangements may be thus summarized. A strong armored tower, with armored wings at its corners, as complete as possible in all its internal communications and appliances, with a clear serviceable bridge or platform round it, from the position of which the captain has full control of the armored tower, and is best placed for maneuvering and fighting his ship.

All iron-clads are at present provided with conning-towers of more or less efficiency, but there is a general want of completeness and uniformity in the arrangements, as a whole, for the serviceable conning and directing of these ships in action. Unarmored ships are almost entirely without any system of protection for their conning arrangements and communications. It would be found extremely beneficial to such vessels were they fitted with towers and bridges similar to those above described, the towers constructed of a moderate thickness of steel, instead of being armored.

As a matter of actual *construction* the conning-tower with its bridge may be considered of comparatively minor importance; but in the *design* of a ship of war its position should be early decided upon, and to it all other external arrangements, whether with reference to guns, funnels, boats, or masts. should be subservient. I have said a good deal on this subject, because it so greatly affects the fighting efficiency of the ship from the captain's point of view.

IV.—*On torpedo defense.*

The necessity of supplying the ships of our fleet with means of protection against the attack of torpedoes is but too apparent, when we consider the gigantic strides that the methods of torpedo attack have recently taken; the immense increase in the number of torpedo craft, some of unparalleled speed; the wonderful results obtained with Whitehead torpedoes; and the fact that every nation with a seaboard is arming itself with these destructive weapons. In maritime warfare, every state whose navy is not sufficiently powerful to keep the sea against its adversary will cultivate the art of attacking with torpedoes, while those nations whose naval power admits of their sending fleets to sea, must study the resistance of such attacks, or, in other words, "torpedo defense."

Torpedoes may be classed in general terms as stationary and locomotive. The former class is practically unlimited in destructive power; such torpedoes must, therefore, be rendered harmless before an iron-clad or other important vessel should be allowed to approach within their range. The latter class is fortunately limited in power, and it is against these weapons that we have principally to provide protection. Apart from any strengthening measures which may be taken in the

construction of the ship below water to enable her to resist the explosion of submarine mines, there are two methods of defense against locomotive torpedoes: one by rigging out obstructions and hanging nets round the ships (this, assisted by machine-gun fire, is the only defense a ship can provide from her own resources), and the other by means of counter-attack vessels of equal or nearly equal speed with torpedo-boats, but capable of keeping the sea and cruising with the fleet.

In a secure anchorage it would be easy enough to arrange effective protection in the way of obstruction, supplemented, if necessary, with lines of torpedoes round the ship or anchorage; but it must be borne in mind that a ship so protected sacrifices her mobility, and may become the victim of a ram before she can get under weigh and be clear for maneuvering. In order to constitute a serviceable defense, nets and obstructions should be capable of being got in and out quickly; they should be strong enough to be carried at sea (when the nets on the beam of the ship might be rigged out if required, provided that the weather was moderate), and the gear should be so arranged as not to mask the guns at any time. Unless this system of defense is most carefully and serviceably fitted, the dangers of fouling the screw, impeding the ship's movements, and otherwise detracting from her fighting efficiency, are such that many officers would sooner risk being without it. However, so great is the importance to England of having her valuable battle-ships protected from the attack of torpedoes, that it may be hoped no pains or expense will be spared in designing and supplying our principal vessels with effective torpedo-net defense.

As regards small sea-going vessels of great speed to accompany the larger and more unwieldy ships of the fleet as auxiliaries, it is fast becoming recognized that the need of some such vessels will be urgently felt in time of war, and that it would be well if every battle-ship in a squadron had one or more of these small craft attached to her, as a tender to protect her from surprise, and, perhaps, the successful attack of swift torpedo boats or vessels, which her own unwieldiness might prevent her from combating. Captain R. H. Harris, R. N., read an admirable paper on the subject, at the United Service Institution, last year.

Some small, swift vessels, commonly known as the Rendel type, have lately been constructed at Sir William Armstrong and Company's works, and proved a great success. The class which is called at Elswick "Cruiser A" consists of vessels of only 1,360 tons displacement, carrying two 25-ton and other small guns, and realizing a maximum speed of no less than 16.4 knots, besides which they are reported to be sea-kind and serviceable craft. To nations whose battle-ships are to keep the sea in time of war, vessels of a similar type—we may almost say—are *indispensable*. Such vessels should be limited to 1,200 tons displacement (or less for the comparatively smooth waters of the Mediterranean), they should carry an armament consisting chiefly of machine shell guns, and torpedoes, and possess the highest speed possible for a vessel intended

for sea-going work. Their three essentials are: (1) Great speed; (2) Small size; (3) Seaworthiness.

The protection against torpedoes which comes most directly under the heading of this paper, is that obtained by giving greater strength to the bottom of the ship, with a view to its resisting the destructive effects of submarine explosions. So long ago as 1869, Sir Edward Reed propounded and established the principle of constructing the hulls of armored ships as lightly as possible, so that a greater proportion of the weight of displacement might be devoted to armor, armament, equipment, &c. Torpedoes were then in their infancy, and the idea was to submit to having the outer skin of the ship blown in by a torpedo, and to trust to double bottom and other compartments to prevent her being seriously damaged. This principle appears to be still in vogue; but since locomotive torpedo attack has now so greatly developed, would it not be prudent to reconsider the question, and put more strength into the construction of the ship's bottom, even at the expense of reducing the weight of armor to be carried? We cannot hope to obtain sufficient strength to resist the effect of a charge of dynamite or gun-cotton ignited when actually in contact, but with stronger bottoms, and perhaps the introduction of some plan by which the blow on the outer skin might be cushioned, a fair amount of safety would be secured against such explosions, when the mine is not in contact. So much has recently been done to organize and perfect the attack on the vulnerable parts of the ship below water, that we may well think seriously of introducing some such modification in the construction of the hulls of our armored ships. Whilst on the general question of defense, we, in common with other nations intending to hold the sea against their adversaries, must be prepared with serviceable, well-fitted nets and obstructions; and counter-attack vessels thoroughly suited for the work; to resist torpedo attack in every variety of form.

From the numerous subjects of interest bearing on the construction of ships of war I have selected these four, because they appear to me to be among the foremost in importance from a naval point of view. The strength of the ram and the system of water-tight compartments are now old questions, but age does not detract from their value, and they must not be allowed to rust, while the necessity of having efficient conning-towers and torpedo protection becomes increasingly apparent in proportion to the improvements in the accuracy and power of heavy ordnance and machine guns, and the development of the attack with torpedoes. I have endeavored to point out where improvements might be convenient, and where weaknesses may be experienced when our ships are tested in action, but I shall be perfectly satisfied if I have succeeded in impressing the meeting with the importance of giving these questions full consideration. I trust some of the gentlemen present will freely, and, if they think fit, severely criticise this paper.

DISCUSSION.

Sir EDWARD REED. My lord and gentlemen, I think no time should be lost, at any rate, by the ship-building members of this society, in recognizing the unusual value of this paper. It appears to me that it is a paper of a very exceptional kind with us, but of precisely the kind which was contemplated from the very foundation of this Institution. It was hoped that one of the great objects which this Institution would effect would be the bringing within reach of each other the ship-builder and the sailor; and I am sure that this paper has brought the sailor in our midst to-day with very considerable advantage on three or four points that must have exercised the minds of all designers of war vessels. I agree with the author in thinking that he has selected some of the most important points demanding consideration, in the choice of subjects which he has made. With regard to his observations on the strength of the bow for ramming, I am inclined to think that the subject requires a little more consideration, as is not unnatural, than he has given to it. In the first place, in the only experience that we have had, at any rate that I have been able to study myself—the only practical experience with ramming—some very curious and unexpected facts were developed, and one which I would recommend to the careful attention of Captain Noel, because I notice that in the paper he suggests that citadel ships are peculiarly liable to weakness and injury at the bow when used for ramming, in consequence of the absence of armor-plates at the bow to back up the stem. Now, that is a very natural opinion to form, but I am sorry to say that in the cases of the ramming which occurred between the two German frigates, it was proved that the presence of armor backing up the stem was precisely the cause, taken in conjunction with the enormous protrusion of the stem below the water, of the great amount of injury which happened. What occurred was this: the ramming ship took the other ship obliquely, and both were at speed, and that being the case the underwater point of the ram, which, as I say, had great advancement, was seized as it were by the ship rammed, and the armor which came to back up the stem at the water-line became a fulcrum, and consequently the momentum of the other ship, acting at the point of the stem, tended to, and did, turn the whole stem about the armor ends. The armor at the water-line served as a fulcrum, and the stem was wrested out from all its connections, and resulted in a most frightful displacement. Now, I only suggest this: it is not at all as an expression of the desirability of having no armor to back up the stem, but rather as an indication that unless the matter is very fully thought out, and thought out in relation to the form of the stem as well as to the fact of the presence of armor, and to the width of the armor which backs up the stem, you might possibly make great mistakes. Another remark touching the ram, in which I cannot quite concur with Captain Noel, is the intimation

which he gives that in his opinion the rams of the composite vessels sheathed with copper and built with brass stems and stern-posts are brittle and weak. I think that they are not by any means brittle, and I very much doubt whether it would be found in action that they are not amply strong enough for all their purposes; I mean regarded as solid blocks of metal; I am not speaking now, of course, of their connection. There is a point to be carefully considered in that connection, and it is, I think, a very serious one, touching that type of ship, and that is that you must expect—at least I expect—that in all cases of one ship ramming another at speed there will be great disturbance at the bow, however well you build it, and when you set up disturbance at the bow of a composite vessel sheathed with copper, you set up an opportunity for the copper bottom attacking the iron of the ship and extending the mischief by chemical action from the moment the ramming takes place. That is a serious point which deserves more consideration, and I confess that for my part I adopted originally in the Inconstant the combination of a copper bottom with an iron-plated bottom with great apprehension, and I view with some objection and some fear its greater extension, and as far as I can I avoid the adoption of copper bottoms with an iron-bottomed ship; I do not like the combination. In saying that, I do not wish at all to say that those do wrong who adopt that combination, although my own feeling is against it. Coming now to the next point (and our Rules make it very difficult to do justice to a paper of this kind in so short a time), I would venture to say that Captain Noel has expressed on the question of water-tight compartments, and the various complications that now occur in connection with the working of a ship, an apprehension which I am not at all surprised at, and one which has given me great anxiety at many times. I very much doubt whether the modern ship of war has not become, by complication in every part, a machine which men cannot efficiently master and handle in action. Take the case of the one ironclad which we lost by collision, the Vanguard, as an illustration of the manner in which even simple and obvious duties are likely to be lost sight of in the hour of battle. How was that ship lost? She was lost from the neglect of the most obvious precaution after her collision occurred, namely, the closing of the water-tight doors between the engine and boiler room; and, moreover, in that case the chief engineer of the ship was in charge of that duty, and it was the occasion when, if upon any occasion, you might have expected the thing to be well done; and yet the responsible officer tells us that when the accident happened he rushed out and saw water pouring into the engine room, and (instead of applying himself to the closing of the water-tight door, which was the one thing to save the ship) he ran on deck and told the captain that in his opinion the ship would go down. Well, with facts like that before us, how can it be expected that men will be found, whether officers or executive seamen, to perform all the things which it will be requisite to perform

in the case of all the accidents of a naval war, and under all the excitements of action? I remember a famous admiral of the American service, Admiral Farragut, speaking to me upon that point, and almost beseeching me—after we had made a visit together to Chatham dockyard on one occasion when I had the honor of being at the Admiralty—to keep things as simple as possible, and warning me not to depend upon either officers or men handling complicated things under such circumstances. Why, my lord, just imagine what must be the nervous condition of any man not gifted with extraordinary nerve in entering upon an action in command of an ironclad ship, with the knowledge that he has half a million or three-quarters of a million of money beneath his feet, together with a great many lives, and that in the only instances in which one big ironclad has rammed another at speed the other has gone to the bottom. I say that the anxieties of naval men in that position must be very great, and I think it most desirable that they should come here and express them to us frankly and fully, as Captain Noel has done in this case, and I think we should lay their suggestions to heart. Now, I should like to say a word or two upon conning-towers. I would like first of all to say that this section of the paper is peculiarly gratifying to me, because it is now twenty years that I have been endeavoring to elicit from naval officers exactly what we have here got. Several times over, when I was at the Admiralty, as I think the right honorable and gallant gentleman sitting there [Admiral Sir John D. Hay, Bart., M. P.] will bear me witness, it was a matter of anxiety on the part of the Admiralty to elicit from the admirals and captains in command of the ships of the Channel squadron suggestions with regard to what ought to be the character of the conning-tower on an ironclad line-of-battle ship, and what we always got was a variety of suggestions, and what we seldom got was a confident opinion and a defined plan; in fact, we never got one, so far as I can remember. I think myself it is a question pre-eminently for the naval officer to tell us how he would like to be situated and circumstanced in action in commanding and fighting one of these ships. Now, I may say that the Admiralty have not been wanting, in my opinion, in this—if my gallant friends present will forgive my saying so—so much as the naval officers. I remember in the case of one ship I put a tower which was broad enough—it was placed abaft the funnel—to enable you to see past the funnel on ahead quite clearly over the bow; you saw everywhere else quite perfectly; it was high enough to look freely over the forecastle, and over the boats and everything else. It was large enough to contain a steering wheel and the various signals, and in fact was what I would call the embryo idea of the more elaborate suggestion which Captain Noel has made; and the only result I got for my pains was that the then senior sea lord of the navy went and rather derided this tower on board the ship, and said how much better it would have been if the weight of that tower had been put in small guns along the

upper deck. That is an illustration of one of the difficulties we have had. I do not much complain of this, my lord, because every one must see that the problem is of the most difficult kind possible, and that it is one upon which naval officers, like ourselves, must of necessity have differed materially during the fluctuating conditions of the naval service in times past. I should like to say, further, that I very much approve of this tower of Captain Noel's. I think most highly of it, and I take a very lively interest in it. I remember having some discussion on this matter with a very eminent officer of a foreign service, the late Prince Adelbert of Prussia, when we were engaged in building the Kaiser and the Deutschland, which Mr. Samuda constructed for that Government, and with which I was connected. We had a great deal of discussion in Berlin about the conning-towers of those ships, and his royal highness came to the conclusion—at least he was very strongly of the opinion which Captain Noel has expressed, namely, that although a tower is most valuable and indispensable for certain purposes, I mean for being within it—it is very necessary to have outside armored shelters, so that officers can move from side to side, and so that they can even meet together in temporary security outside of the tower under many of the conditions of naval warfare. Without further dwelling upon it, I would say that, as far as I can see, the tower which Captain Noel suggests is a most admirable one, and thoroughly well deserving the consideration of all of us who are engaged in works of this nature. I am afraid that I have taken up my ten minutes, but I should like to make one remark upon the subject last touched upon in the paper, that of torpedo defense. Captain Noel says that as long ago as 1869, and he would have been quite correct if he had said as long ago as 1863, I assisted, or at least took part, in establishing the principle of saving as much iron as possible in the merely floating bottom of the vessel to get it for the armor and other things; and a good deal of trouble we got into, because people who are often more ingenious than they are kind, discovered, in the fact that we had saved a little more than we had calculated upon in the one case, and required a little ballast to replace it, a reason for attacking a class of ships and their designers incessantly, and misrepresenting them in every possible way; and not only that, but for turning what I believe was a professional triumph into a means of professional discredit and disadvantage. But I mention this point for the purpose of saying that my thoughts run rather in a different direction, and towards a different result; I do not know that they will continue to do so, but they do at present, and have done for some time past, from that which Captain Noel suggests. The idea is that we should make this outer bottom of the ship thicker. Now, my idea is that whatever we can do in the way of thickening the bottom of the ship, it should be the inner bottom that we make thicker, and that we might make the outer bottom thinner with advantage in certain cases, giving the iron so saved to the inner bottom; and my reason is this: That the inner

bottom is further away from the torpedo than the outer one, under whatever circumstances the torpedo is exploded, and the further you can get the torpedo to explode from your strong bottom the better. Therefore I incline to think that it is possible we may come to this; we may come to an internal bottom, which would be much stronger than bottoms now are, and that we should make the external bottom—not a net, of course, that would not be possible—but such as to perform the function which the net performs at some distance from the ship, to perform that at some distance from the inner bottom of the ship. I do not put these views forward as at all final or conclusive; they are merely suggestions which have occurred to my own mind. Having exhausted my time, I will only repeat, my lord, that I for one am extremely pleased with this paper, and very grateful to Captain Noel for having read it.

Mr. J. D'A. SAMUDA. My lord, I entirely concur in all the observations which Sir Edward Reed has made with reference to the ram, and I would merely add in confirmation of what he has stated that the apprehension expressed by Captain Noel in this paper may be quite put on one side with reference to the material of gun-metal not being suitable for its application in the position of the ram when wood and copper have to be used in conjunction with a steel-built vessel. The difference in strength between properly constructed gun-metal—properly mixed gun-metal—and iron is very much less than Captain Noel could have imagined when he wrote this paper. I am testing some myself at this very moment on a ram of the particular kind which he has described, and he will possibly hardly believe it when I tell him that the breaking strain of that gun-metal has been shown to be over 18 tons to the inch (that of ordinary iron being 22 tons), besides having a good deal more elasticity. Therefore, I think he may altogether disregard the idea that gun-metal will have any bad influence whenever it is desired to use it in the construction of vessels of this sort. I do not think that this would be perhaps quite the right time to go into one's views with regard to the application of the ram generally as an offensive power in the navy; but for my own part I hold that ramming will be very little used and very much less successful than guns when it is used; and you will find, as a rule, that what Captain Noel has expressed here is again quite right, that it is not a science that can be practiced very much and that, consequently, deductions cannot be drawn from it very easily. As to the extent to which it has been used, Sir Edward Reed has mentioned one case to which I do not care to allude again, but I should just like to mention another in confirmation of the view I have expressed with regard to it, and that I received from Admiral Tegetthoff, who, I believe, is the only man who has used the ram in actual warfare. He described to me in the most graphic way the effect produced upon him by having rammed the Ré d'Italia which you will recollect was sent to the bottom by his vessel at the battle of Lissa. He said, "If I were to live a thousand

years I never would ram another ship, the impression produced upon me was so awful." It was not only the effect produced upon him as being singular, because my own impression is that he was one of the coolest and bravest men I ever spoke to; but he said, "I went into that action having given orders to every captain under me, when I made a certain signal, to turn his vessel and ram the vessel opposite to him, and the only one that rammed was my own. After I had done it the effect produced was different from anything you have in naval warfare generally. You see the vessel attacked at one moment, and the next moment after she is struck 800 men sliding noiselessly into the sea, with the vessel following them, and then find yourself left with a perfect void, without any commotion, without any smoke, without anything to make one feel that one was in battle. All these things created such an impression upon me that under no circumstances would I ever ram again." Well, I cannot think, when a successful and brave man has dealt with the matter in that way, that men feeling that great amount of responsibility upon them would ever use that power when they have got what I consider to be a very much greater power in their possession; that is, good guns to make use of at a distance, and in the ordinary mode of warfare. However, perhaps, that is an observation which I ought not to have made in discussing this paper, because that is a matter which is peculiarly within the province of the naval officer, and not of the constructor of vessels. I would like to say, just in further confirmation of this paper that I entirely agree with the general character of the line taken by the paper as to meeting torpedo defense. I believe that torpedo defense must be met by auxiliary vessels acting in conjunction with these large vessels which we are now building and relying upon. What the size of these vessels that will accompany the fleet will be is a matter of some considerable doubt at the present moment. Captain Noel has fixed it at about 1,000 to 1,200 tons, and though when acting in confined waters smaller vessels would be useful, yet in many seas these would be none too large. There is, I see, an interesting paper coming on immediately in this present session, in which M. Normand thinks that something like 50 tons displacement would be sufficient for vessels carrying out that work. I cannot at all agree to this. I think Captain Noel's figure is very much nearer the point than M. Norman's. I am quite sure that you do require, with vessels costing, as Sir Edward Reed has observed, over half a million of money, to have auxiliary vessels acting in conjunction with you that can, like scouts or like cavalry, come out and keep off such attacks as can be made upon you, and at the same time can possess great speed and that amount of gunpower that they can destroy vessels, be they large or small—generally speaking they will be small—so as to leave the larger ship in its more natural position of fighting vessels of its own size and vessels armed with great power. That I think is indispensable. I do not think that a torpedo-boat such as can be placed in the davits suitable for this service

at all. I am speaking of vessels which are sufficiently large to keep the sea in company with the fleet and of coming out and acting in conjunction with the fleet on those particular occasions. All that, I think, has been very admirably explained in this paper; and without going into any other points, I think we ought to give the thanks of this Institution to Captain Noel for having given us the fighting views of the captains in connection with this matter.

Mr. N. BARNABY. I should like, my lord, to say one word, or not much more, upon this paper. Sir Edward Reed has taken up the points which I had marked down here as being, as I thought, worthy of some remark, and I have scarcely anything to add upon those which he has touched, except that I would remind him—and he will be glad to be reminded of it—that there have been three or four cases of accidental ramming between ironclads belonging to Her Majesty's Navy, where ships have been struck and have not gone to the bottom. With regard to the strength of the outer bottom, Sir Edward Reed will remember what a keen discussion there was concerning the strength of the bottoms of some of the early ships that he has had in his mind. One of them in particular, members here will recollect, was the Iron Duke. One of the contentions of Sir Edward Reed was (and with that I always fully agreed) that the strength of the outer bottom in that ship was quite sufficient, and in fact she was a better ship than if she had been made stiffer. I do not see the late captain of the Iron Duke in the room, or I am sure he would be ready to confirm what I am going to say, that, unfortunately, the Iron Duke was badly ashore twice; that on one occasion she sued half her draught of water and received no injury; that on the other she got upon the rocks and tore open the outer bottom, and that no water got into the ship at all from the inner bottom, except in one place where a stiff engineer's tube had been forced in through its gland, and the water found its way in by that passage. The outer bottom was torn open; the frames between the two bottoms were crushed, and there could not have been a better illustration, I think, than that ship furnished of the justice of the contention of the Admiralty officers in defending that class of ships. The question of the defense of that class is now a matter of history, but it is important as affecting our present practice. And I would refer therefore to one other ship about which we were told that she certainly was too slight—namely the Iris. Unfortunately the Iris has been ashore quite recently, and the letters we received from the dock-yard officers at Malta, where she has been docked, are to the effect that the ship has behaved splendidly; that the outer bottom is considerably injured—or rather I will not say considerably injured, but there are injuries to the outer bottom all along the length of the ship. I withdraw the word considerably, and say that the bottom is broken through to a certain extent, but that the defect can be made good very easily and in a very short time, and the ship appears to have never been in the least danger of having water

admitted into the ship proper—that is, inside the inner bottom. One other word as to "armored tonnage" and the small vessels to which Captain Noel has referred, and about which Mr. Samuda has just spoken. We have had a great many discussions here of importance on this subject, and my own views are quite in accordance with theirs, so far as they are worth anything. But there are very large demands made by Parliament—very rightly, no doubt—for armored tonnage, and these vessels not being armored, strictly speaking, do not count. So long as the strong demands are formulated for armored tonnage, and the navy estimates remain where they are, the chance of getting these most important vessels is extremely remote.

Mr. W. H. WHITE. My lord, I wish first of all to join in the expression of thanks to Captain Noel for so fairly giving us his views. I would like to add, that I sincerely wish we had had the opportunity of having one or more papers from other naval officers, because then we should have had illustrated very forcibly, what is the fact, that different naval officers have very different opinions on the same points as affecting the management and efficiency of ships. From the nature of the problem, as Sir Edward Reed has suggested, that is inevitable, and I think it is quite proper; but speaking generally, and confining myself to Captain Noel's opinion, I think, if he will excuse my saying so, that he has not fully considered the difference between what may be called the provision for the peace conditions of living in a ship and the conditions of fighting her in action. Now, the designers of ships have to think of both. It is true that the more important is the condition for action, but the condition for action is one to be reached by preparation. To take an illustration, I think that scarcely any naval architect would put a water-tight door in a bulkhead if he thought that the ship could be worked without complaint without such a fitting; but, speaking now from an experience of many years, and I know I shall be very fully confirmed by everybody who has built war ships, the difficulty is to keep out water-tight doors. You start with what is considered the minimum number, the ship proceeds, and the tendency always is to increase the number. For instance, it is difficult to pass along at a certain level in the ship; the suggestion is made at once to put in a water-tight door. It can always be closed. The necessity for closing it in action will come once in twenty years, perhaps, but the desirability of having it there is of every-day occurrence. That is one out of many illustrations that might be given of the way in which the peace conditions of every-day service interfere with readiness for action. And if I may be allowed, just following up the remarks that have been made as to the complication of ships and the impossibility of overtaking that complication under the conditions of action, it seems to me, from my observations upon the subject, that there is always a pretty safe assurance that drill will come up to fittings. To take again water-tight doors, the Vanguard disaster was one which we have not altogether

reason to regret. It is of course to be regretted, but it has its good side. I am speaking again as an observer merely, and Captain Noel will confirm me in this; the drill of the navy has greatly improved from that time in closing water-tight doors quickly, dealing with large openings on the outside of the ship, and so on—all these things have received the greatest attention in the service since, and the drill of the navy has overtaken the necessities of the case. So I believe it will always be. Perhaps the designers of ships err in one direction; they may tend to greater details in fittings, greater complications, more mechanical refinements, than the sailor feels comfortable with—and I can only say I am very thankful I have not to manage a modern ironclad—but, at the same time, I think there is a rectification continually proceeding, that is to say, the instructed officers of the navy are continually overtaking their responsibilities, and I believe they always will. It must be remembered that these fittings are always subject to naval criticism in all their stages. They are not put in at the whim of any ship-designer simply; they are put in after conference between the designer of the ship and the naval officers. Captain Noel will be glad to hear that some of the recommendations which he makes in the paper are established practices in recent ships. For example, we will take this which he has mentioned, and to which he has very properly attached importance, the question of escapes from spaces which must be occupied in action. In modern ships these are provided. Perhaps it is possible to make it too easy for a man to run away. I do not know whether that is so, but at all events in all cases where good reason can be shown, and where it is possible to do so, the conditions which Captain Noel has formulated on page 5, as to providing for a man escaping from the torpedo compartments, the stokehold and so on, are very carefully considered, and almost on the lines he has indicated. I should like to add one word on the pilot question, if I may. It is to ask Captain Noel if he has in the least degree considered what weight would be involved in giving anything like efficient protection, that is, a decent thickness of armor, under many of the conditions where this conning-tower might be needed. It is not merely the position in which the weight is to be carried, it is the question of the weight itself which is important often, and that is true, of course, in relation to the size of the ship and its other qualities. I am not expressing any personal opinion about conning-towers at all; I am not quite clear that I have one. I have many, it seems to me, in relation to the various classes of ships to which that protection should or should not be given. But although there is no standard pattern of conning-tower for all classes, it appears to me that it is always a question of the balance of advantage. Can you afford to give up the weight which would be required? and it will be a considerable weight, I think, with such a tower as this. Of course, many naval officers say, "We should be content with protection against machine-gun fire; we do not require armor at all." Others say, "We

desire to have strong armor protection." Some again say, "We should like to go inside the towers," and others "outside the towers." For those who actually have to determine whether they will put a tower or not in a particular class of ship, such a difference of opinion involves very great difficulties in decision. I do not think that Captain Noel, if he were to work this out with a 12-inch thickness of armor, for a common type of ship, such as is now built, would find that he would get this tower for less than twice as much as has ever been put into a pilot-tower yet. That may be quite a wise distribution of weight, but I would be glad to know if he has considered it from that point of view.

Vice-Admiral DE HORSEY. My lord, I rise to contribute my thanks to Captain Noel for the very interesting lecture he has given us, and beg to be allowed to say a word or two with regard to Mr. White's last remark as to whether we should have armor-plated conning-towers or not. I think the majority of my brother officers will be very glad to have an armored conning-tower if it can be given without introducing too much weight, and therefore without sacrificing other necessary things. I am of opinion, myself, that, viewing the great necessity for economizing weight, which is so well known to Sir Edward Reed and others, the enormous weight, as Mr. White has pointed out, that is necessary to make an armored conning-tower of the required size cannot well be afforded, and especially at so high a point. We know the necessity for keeping our weights down as low as possible in our armor-clad ships, which are all, more or less—and must be—top-heavy. I think that most naval officers (I for one, certainly) would be content with a conning-tower which is proof against machine guns. You are thus enabled to make it larger, so as to give the necessary space for directing your guns, and your helm, and your torpedoes; but if you at a pinch the size of the conning-tower in order to economize weight, which a constructor would be very apt to do, then the conning-tower loses enormously in value. One word I should like to say, my lord, with reference to Mr. Samuda's excellent exposition of ramming. I cannot agree with him (and I think the lecturer will be of my opinion) in thinking that ramming is not an important thing. I look upon it as a most important mode of attack, and I do not think Mr. Samuda could have put its importance better than when he so graphically explained to you how in a moment you could send 800 people to the bottom. I think you will find, my lord, that no naval officer will hesitate for one moment, if he gets an opportunity, to avail himself of such an admirable mode of dealing with an enemy.

Admiral Sir COOPER KEY. Before Captain Noel replies upon this discussion, may I say one word which may perhaps help him in his answer. It is only on the important question of conning-towers. Mr. White asked the very difficult question, What would be the weight of the tower he proposes? I just wish to say that I do not think 12 inches of armor are nearly sufficient for a conning-tower. I think if you have

only 12 inches you had better do away with it altogether. If you are to have a secure conning-tower it should be at least 15 or 16 inches thick. I hope Captain Noel will give us the weight he considers necessary for a tower of that description.

Captain Noel. My lord and gentlemen, I have to thank you very sincerely for the manner in which the paper has been received. I wish I could have put more value into it, but I was very much pushed for time. I was asked in January whether I would read a paper, and having a few notes with me, I put this together. I wish you to understand that in this paper I speak for myself as a naval officer, and not in any way in behalf of the navy. In the discussion we have had some interesting points raised, and I had perhaps better go through them in order. First, as regards the strength of the ram. Sir Edward Reed brought forward a question with respect to the bow of the König Wilhelm, and, according to his statement, the armor in that case did not support the bow; but I think we may say that the bow of the König Wilhelm is one that has great weaknesses, one of a type not to be found in our service. Our bows are not so long, they do not project so far; such projection is alone a matter of weakness. The bow of the König Wilhelm extends very far from the main part of the ship; it is also a very narrow bow; it has no root, which I ventured to suggest was requisite; it is a sharp angular bow, which naturally gets very little support from the armor. I brought up the question of armor supporting the ram on the authority of the lamented friend of the institution, Mr. Scott Russell, who talked of the bow being built of thick smooth plates, all supporting each other. But to get strength in the bow I take it you require a *root*, a firm structure on which to build your bow, which is in itself inflexible and cannot be easily turned; then on that root I fancy that armor would certainly be a help. As regards brass stems, I have met with much opposition on this question. It may be that I am not sufficiently well informed, but the idea that occurs to me is this: it is not a matter of the tensile strength of brass, it is the power it has to resist a sheering force. Now, in ramming, the ram point is generally made so as to strike a vessel below her line of armor, and the greatest strain will come on that part of the stem which takes the armor shelf, and that will be essentially a sheering strain. I rather incline to hold my point, that brass would not stand that sheering strain in the same manner as wrought iron or steel. As regards ramming, Admiral Tegetthoff was one of the greatest authorities on the subject. He was the first man in the field to show us what ramming could do. Naval officers, however, must not listen to Mr. Samuda, or rather to what Mr. Samuda has told us about Admiral Tegetthoff. They must not lose heart. The thing is, that if we do not ram we shall be rammed, and therefore in self-defense we must ram. And, as I have stated before, in a pamphlet I wrote some time ago, if you intend to ram your enemy and you keep your bows on to her and at the last moment flinch, she will ram you, unless she is badly man-

aged; of course, on the other hand, if she flinches, you ram her. But the tactics of ramming are a science which I am certain (when we do have the war which must come some day) will be very important. Ramming is a thing that we cannot shirk, and for that reason our stems should be fit for the work. As regards water-tight compartments we shall hear more, I think, in the next paper. I quite agree with Mr. White, that it is very often the naval officer who wants the openings; but, all the same, they are most dangerous things. There is little doubt of this, that that very ship, the König Wilhelm, when she rammed and smashed her ram in, would have sunk had she not had an intact bulkhead forward; there happened to be an intact bulkhead some 50 feet from the bow, which kept the water from filling the ship. I am very pleased to hear that there are modes of escape from below; I did not doubt that the matter had been considered. I went over the Polyphemus one day, and I felt that I should not like to live in the stokehold; for when the ship was bumping about between ramming, sending off torpedoes, explosions occurring on board, and one thing and another, he would be a plucky stoker who would stick to his work. As regards the conning-tower, I am exceedingly glad to find that it has created interest in the meeting. Conning-towers I have looked upon as one of the most important things in the fighting efficiency of a ship. I have compared a ship without an efficient conning-tower to a man without an efficient head-piece; I have not said efficient brains, because it might be remarked, "Oh, you mean that the captain represents the brains, and therefore you want to protect the captain." Now, it is not protection for the captain that I want. If the captain suffers there is another man to take his place, but if the communications suffer the ship is done for. It is the communications that we want to protect. The communications are the nerves, the ship being the body, and the protection of those communications is a vital necessity for the ship. Therefore I do not care what weight is given; let the weight be what it may, take that weight from the armor, or take it from the guns, take it from anything, but let us have the communications so protected that we can fight our ships. The Huascar had her tower knocked away the first thing, and there she was helpless in the action with the Chilian vessels.

Vice-Admiral DE HORSEY. It was an armor-clad vessel.

Captain NOEL. She was armored, but very poorly armored. Still, the thing is to keep the communications clear and under good protection. Of course, viewing this plan, it looks as if the conning-tower is a very large business, but the lower part of the conning-tower need only be an armored funnel or tube. This also must be borne in mind: that a structure of an oval or oblong shape need not be of such very great thickness. With all due deference to Sir Cooper Key, I think nine or ten inches in that shape is almost sufficient; at all events, I should limit it to twelve inches in most cases. Then comes another thing. If the position is well chosen—all these points are considerations for the

designers of ships—the conning-tower should hardly be visible from outside. The bridge netting can be built in with the nettings of the ship. So long as the conning-tower looks over everything, that is all that is necessary. You do not see the conning-tower as a large tower standing up in the ship; you merely see it as shown in the plan, a foot and a half above the netting. The bridge netting would probably be a little above the ordinary netting of the ship, and other obstacles should be kept below that. Therefore, the conning-tower is not an object very easily observed from outside. As to torpedo defense, it is of course entirely a matter for the constructors, to decide how to get the necessary strength to resist torpedoes in the construction of the bottoms of ships; whether with weak outer bottoms and thick inner bottoms or *vice versa*, or by some other plan. But I think there is very little question that, seeing how greatly torpedo attack has improved in every way, and how the attack by torpedoes is likely to be cultivated by almost every nation that has a seaboard, we should pay particular attention to the subject. I think that the meeting, as far as we have heard, agrees with the idea (which was not mine at all) of the necessity of having torpedo vessels of great speed, and I am very glad to be supported by Mr. Samuda as regards the tonnage; I do not think we shall get thoroughly efficient sea-going vessels very much under 1,000 tons. Mr. White has said a good deal about peace considerations. Now, I hold that war considerations are far more important than peace considerations, and I imagine that naval officers are willing to sacrifice a good deal to know that their ships are thoroughly efficient for being taken into action. I do not think I have anything more to add.

The PRESIDENT. I am quite sure you will allow me to convey to Captain Noel your united thanks for his most valuable paper, and to thank those gentlemen who joined in the discussion for their remarks. I am perfectly sure of this, gentlemen, that there is no class of criticism that the designer, the naval architect, hails with more thorough satisfaction than naval criticism, because he brings his study and his science to bear upon the machine, but the naval officer uses it, and consequently it is well—it is essential, in fact—for the success of our ship-building, that these criticisms should be given, and it is upon that account that I am extremely gratified to-day to see so large and influential and able an attendance of naval officers. I only hope it is the beginning of better times, and we shall have more naval officers attending our sittings than we sometimes have had in the past. Perhaps, gentlemen, you will allow me to convey your cordial thanks to Captain Noel.

VII.

EFFICIENCY OF GUIDE-BLADE PROPELLERS.

By J. I. THORNYCRAFT, Esq., *Member of Council.*

[Read at the twenty-fourth session of the Institution of Naval Architects, March 14, 1883; the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

During the years 1879 and 1880 I made experiments with guide-blade and other propellers, using models of small dimensions.

These models indicated some advantages to be derived from the use of guide-blade propellers, and my firm has since fitted H. M. torpedo vessel Lightning with a propeller of that kind, and built a shallow steamer for the Congo, with a hull specially formed to suit the requirements of the propeller when used for very shallow draught. I now propose to give a short account of the results obtained with the models, and afterwards with the propellers as fitted to the vessels before named.

The models used were from 5 to 11 inches diameter, and were adapted to use from ½ to 1 horse-power, at a speed of 4½ knots.

In order to experiment with these models, a launch was fitted with a small shaft projecting directly forward from the bow into water which might be considered as almost undisturbed by the motion of the launch through the water, and the small shaft was driven by an engine with suitable gearing, to allow the turning moment exerted on the shaft to be continuously recorded, the shaft at the same time being free to move lengthwise a short distance, without hindrance, and allow the thrust of the propeller to be measured simultaneously.

The launch was propelled principally by another engine, driving a screw at the stern, and the speed of this latter was found to measure the speed of the launch very nearly. A distance of 300 feet was measured on the bank of the river; the time running this distance, the revolutions of the main and experimental propellers, the turning moment and thrust of the model, were all recorded on a sheet of paper held on a drum, which was independently driven, while short intervals of time were marked on the paper by a clock. A great many experiments were made with this apparatus, which was found to work well, and in order to compare the efficiency of the guide-blade models with simple screws under as nearly as possible similar conditions, models of screws were made of larger diameter than their rivals, in a proportion that would use about the same power when working at their best speed. The results obtained will be found in the Table No. I.

TABLE I.—*Dimensions of model propellers and results obtained.*

Date of experiment.	Propeller. Name.	Propeller. Type.	Propeller. Diameter.	Pitch on forward edge.	Pitch on after edge.	Number of blades.	Length of blades.	Obliquity of guides.	Number of guides.	Length of guides.	Length of casing.	Diameter of boss.	Length of body from maximum diameter.	Velocity due to pitch of leading edge, in knots.	Thrust due to 12″ diameter.	Revolutions at maximum efficiency.	Thrust at maximum efficiency.	Maximum efficiency.
																	Pounds.	
July, 1879	No. 2	Common.	7¾″	10½″	10½″	3	1½″	......	......	......	......	2¼″	......	6.04	12.6	700	5¼	.659
July, 1880	No. 11		11.32″	14″	14″	3	......	......	......	......	......	2¼″	......	5.17	12.1	450	10¾	.616
July, 1879	No. 3	Thornycroft first patent.	9″	9.42″	11.14″	3	1″	......	......	......	......	2¼″	......	4.65	12	600	6¾	.705
	No. 3₁		9″	9.42″	11.14″	3	1″	......	......	......	......	2¼″	......	......	......	1,100	17½	.49
June, 1880	No. 10		11.32″	11″	13.64″	3	2¼″	......	......	......	......	2¼″	......	4.62	16.3	510	14½	.635
July, 1880	No. 10₂		11.32″	12″	15″	3	2¼″	......	......	......	......	2¼″	......	4.7	23.4	475	20½	.63
July, 1880	No. 10₃		11.32″	11″	14½″	3	2¼″	......	......	......	......	2¼″	......	4.43	16.8	490	15	.614
Aug. 1880	No. 10₄		11.32″	11″	13.64″	3	2¼″	......	......	......	......	4½″	7″	5.2	22.5	575	20	.56
Aug. 1880	No. 10₅		11.32″	11″	13.64″	3	2¼″	......	......	......	......	3	3¼″	4.75	19.1	525	17	.63
Aug. 1880	No. 10		11.32″	11″	13.64″	3	2¼″	......	......	......	......	2¼″	......	4.6	16.3	510	14½	.64
Sept. 1879	No. 6₄	Thornycroft second patent.	8″	10″	19½″	2	3½″	⅛″	6	2″	4¾″	4½″	12¾″	4.1	31.5	500	14	.665
Sept. 1879	No. 6₅		8	10″	19½″	3	3½″	⅛″	6	2″	4¾″	4½″	12¾″	3.62	26.4	440	11¾	.663
Nov. 1879	No. 7₁		8⅜″	17.75″	21.25″	3	2¼″	¼″	6	1¾″	3¼″	4½″	12¾″	5.83	21	400	11½	.645
April, 1880	No. 9₀		8″	12.23″	71.6″	2	5″	7/16″	6	1¾″	6¾″	4½″	12¾″	3.52	32.6	350	14½	.577
April, 1880	No. 9₂		8″	12.23″	71.6″	4	5″	7/16″	6	1¾″	6¾″	4½″	12¾″	3.77	50.6	375	22½	.53
June, 1880	No. 6₉		8″	10″	19½″	2	3½″	⅜″	6	1 1/16″	6¼″	4½″	8″	3.7	22.5	450	10	.644

In this table the particulars of some of the propellers tried are arranged for each propeller in horizontal lines and named by a number by which each propeller will be known. Numbers 2 and 11 are described as common propellers, and are of uniform pitch throughout their propelling surface, having an oval-shaped blade as in common use, and these were made for comparison with the other propellers tried as a sort of standard, by which the merits of the other propellers could be measured.

Numbers 3 and 10 are screw propellers, with the blades thrown back, and the radial center-line of the blade is convex on the driving face. These are described on the diagram table as Thornycroft No. 1. Numbers 6, 7, and 9 are guide-blade propellers, having blades and guides much like those proposed by the Hon. Richard Parsons, but having also another feature which is important to insure success. This consists in a large boss, which gradually contracts the area of the stream flowing through the propeller, and is followed by a body which gradually allows the accelerated stream to unite.

This table only contains the results of a portion of the models tried, but they have been selected as being those of the greatest interest, and giving the highest efficiency. The table will, I hope, explain itself for the most part, but there are some terms used which require explanation. I believe Mr. Froude was the first to show that there was a particular speed of running for any new screw propeller which gave the best results, and that this speed corresponded to that which gave about 20 per cent. slip. This speed of turning is recorded on the table in the line marked "revolutions at maximum efficiency," and is calculated for a speed of 4½ knots, at which speed the launch was run in the experiments given.

The column "thrust at maximum efficiency" is the thrust of the propeller available for propulsion at the before-named rate of turning and speed through the water. But the figures in this column refer to propellers of various diameters, and cannot therefore directly give any relative idea of the thrust of the several propellers. Another column has therefore been prepared, marked "thrust due to 12 inches diameter," and gives the relative performance when the diameters are equal.

The column marked "velocity due to pitch of leading edge" is of interest, as it shows how far the idea has been realized of making the leading edge of the propeller cut the water without striking it.

In comparing the three types of propellers experimented on, it will be seen that the efficiency does not vary much between them, the lowest being the simple screw with uniform pitch. The experiments, however, do not show clearly which propeller should take the highest place in efficiency, for although No. 3 propeller gives .705, No. 10 of the same type gives only .635 on one hand, and .64 when tried again; while Nos. 6_4, 6_5 and 6_9 give, respectively, .665, .663, and .644.

The propeller No. 3 had long narrow blades, which were inconvenient,

and did not allow the engines to be run as fast as has since been found necessary to fully utilize the capacity of the engines in the first-class torpedo-boats, which are now fitted with propellers having wider and shorter blades, giving greater speed for the same boat and engines.

No. 10 is a model of the propeller now used in the second-class torpedo-boats built by my firm, and was tried against a model of the common screw of the same size, No. 11 in the table; the result being considerably in favor of No. 10, which gave an efficiency of over .635.

Propellers similar in form to these models were tried, on second-class boat No. 104, and the results obtained with the boat verified the model experiments on these two propellers.

In the comparison of efficiency, if we may exclude No. 3 and take only those propellers that may be run at a high turning velocity, the guide-blade propellers are the best, and Nos. 6_4 6_5 give above .66. These propellers require to run too fast for the engines if fitted to the first-class torpedo-boats, and the Lightning was fitted with one of the form of which 6_9 is a model, the efficiency in this case being about .64.

Having found that the guide-blade propeller, fitted with large boss to contract the stream, gave such good results, I made some experiments with bosses and bodies fitted to the propeller No. 10; but the results obtained were unsatisfactory, and I was obliged to abandon the hope that the high duty obtained from the guide-blade propeller shown could be obtained by the much more simple means of fitting a suitable body behind a large boss.

The experiments on No. 3_1 in table indicate the same propeller as No. 3, but the trim of the experimental launch was altered so as to allow the ends of the blades to just break the surface of the water, as described by Professor Reynolds in one of his papers read at this Institution. The result in the experiment I made is, I now find, worse than I had imagined, for although, as I have previously stated at the Institution of Civil Engineers, there is a point reached at a very high number of revolutions when the efficiency reaches about .49, the thrust is then very high, and to obtain this result in an actual ship and propeller would be far beyond the capacity of the engines of the vessel.

If this propeller is used to do the amount of work it can do best, when properly immersed (namely, exert a thrust of $6\frac{3}{4}$ pounds at $4\frac{1}{2}$ knots), when breaking the surface of the water and giving the same thrust, it will require to run at 910 revolutions instead of 600, and the efficiency will be only .36, or about half its proper value for the propeller. It is of course uncertain what effect increase of size has on the amount of this loss, which it probably diminishes, but great improvement is required, or the loss of efficiency will still be very serious, even in large propellers.

In examining the velocity due to the leading edge of the various propellers, No. 10 has a slight excess over the undisturbed stream, which stream is equal to $4\frac{1}{2}$ knots, and in No. 10_3, in which the form is changed

so as to give a greater increase of pitch in the width of the blade, the speed of the forward edge is less than $4\frac{1}{2}$ knots, but the efficiency is reduced. This may perhaps be explained by the action a screw has on the water which is immediately in front of its disc; the water is here accelerated by the loss of pressure the blades cause, and arrives at the propeller with almost as high a velocity as it leaves it. In my guide-blade propeller this cannot be the case. There must be an increase of velocity within the instrument due to the amount of contraction given by the boss.

In the guide-blade propeller the speed of forward edge is greatly less than the initial speed of the stream in which they work, except in one of large diameter (No. 7) designed for a lower thrust, the contraction of the stream being less as the same size of boss was placed in a larger tube. As, however, lessening the contraction did not appear to bring any improvement, the opposite course was tried by again using the same size of boss, but in a smaller tube. This also resulted in a reduction of efficiency, although, as might be expected, a very high thrust was obtained.

It would therefore appear that the amount of contraction obtained in the models 6 and 9 is that best suited to propellers where a high efficiency is required, which cannot be sacrificed to give increasd thrust.

I am not prepared to say the figures given represent the exact efficiency of the models tried, but I do believe they may be trusted as to the relative value.

The apparatus used was frequently tested, but the amount of friction in the propeller shaft was always considerable, and unfortunately variable in amount to some extent, and thus liable to lead to error. The launch no doubt disturbed the water slightly, but this would affect all the models to about the same extent.

I have now to ask your attention to the Table No. 2, which gives some particulars of the Lightning. The first column relates to the vessel when fitted with No. 3 propeller, and the other columns to the same vessel as afterwards fitted with No. 6_9 propeller, No. 6_5 in the second column having three blades instead of two.

It will be observed that the coefficient obtained with the new propeller is good except in the last experiment, and I am informed that the Admiralty intend repeating this experiment, as there appeared to be some considerable falling off in speed during this trial.

With regard to steering, the improvement is very marked, and the power of turning the boat against any extra resistance, which the trials do not show, is even more marked.

I would call attention also to the great reduction in the diameter of the propeller used, 5 feet 10 inches reduced to 3 feet.

There is some doubt as to the original propeller being fully immersed even at full speed. This makes the comparison possibly unfair for the

No. 3 propeller, but there was no appearance of racing except when the rudder was put over to a considerable angle.

The arrangement of the propeller as fitted to the Lightning is shown in Figs. 1, 2, 3 (Plate II), where the propeller is inclosed in a tube which carries the guide blades within its after-end, and the part which has been described as the body is carried by the rudder, of which it forms a part. In order to insure ample steering power, the tube was fitted with two curved pieces fitting against the outside on either side, and these were so actuated as to come out and form an additional rudder when the helm was put over to any considerable angle, remaining in their places, however, for small angles of the tiller.

As may be seen from the drawing, this involved complicated mechanism, which in itself is undesirable; and, further, the increased thickness of the tube necessary for this construction increased the resistance of the propelling apparatus, and thereby damaged its efficiency.

Plate III represents a shallow river steamer, propelled by two guide-blade propellers, the hull being specially formed to adapt the propellers to a very shallow draught. A hull of such dimensions has a very large immersed surface for the displacement, being in this case 61.3 square feet per ton, and very unfavorable to the displacement coefficient. When this fact is allowed for, I think we must consider that this boat gives a good result; but the most curious thing connected with this shallow steamer is this: When one engine and propeller only are used, and the other propeller is still on the boat, and not turning, the performance of the vessel with one propeller appears to be better than when both are used; at the same time, their very small diameter must be remarked, and also the moderate speed of engines required. But in order to attain this last result, it was necessary to use a form of blades with exceedingly long pitch (see Table I, propeller No. 9_0); this entailed reduced efficiency only .577, and rendered the action of the propeller very imperfect when going astern, but the way of the boat could be stopped in about two lengths.

The mean pitch in a guide-blade propeller does not give a correct idea of the acceleration to be expected, and the same is true of any screw with a great increase of pitch. It is necessary, in order to compute the action of one portion of the surface, to consider the effect that has been previously produced by the surface that has already acted on a particle of the water, and to illustrate this Diagram 3 has been prepared.

I consider that there are two kinds of vessel where the guide-blade propeller might be used with advantage: they are sea-going vessels which often run in ballast, and, consequently, in their light trim do not properly immerse a common propeller; and the other vessels for navigation where the draught of water is limited and necessitates the use of paddles or guide-blades. In this case it would appear that the guide-blade propeller may be more advantageously used than the paddle where high speed is required, owing to the reduction in the weight of

the machinery, consequent on the increased number of revolutions that may be used, at the same time retaining the light draught due to the paddle-wheel, and having the further advantage of not increasing the extreme width of the vessel, which is an important feature in inland navigation.

In conclusion, I have the pleasure to acknowledge the assistance given me by Mr. Sidney Barnaby in making the experiments, which form the most important part of this paper, and to thank my other assistants for the great pains and care with which they worked out the results.

TABLE II.—*Her Majesty's steam torpedo vessel Lightning.*

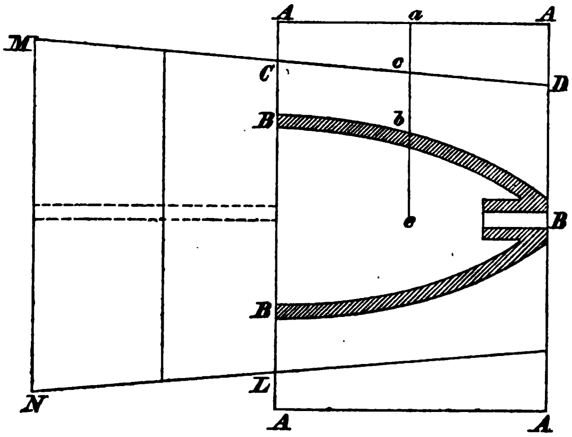
	Stokes Bay.	Thames.		Stokes Bay.
	May 22, 1877, No. 3 propeller.	No. 6_8 propeller, April 26, 1881.	No. 6_9 propeller, June 2, 1881.	January, 1883, No. 6_9 propeller.
Displacement	28 tons	34 tons, about	34 tons, about	34 tons.
Indicated H. P.	400.8	384	477	428
$\frac{V^3 D^{\frac{2}{3}}}{\text{I. H. P.}}$	147	151	151	113
Revolutions of engines per minute	354	390	423	
Speed in knots	18.54	17.7	19.02	16.65
Time required to make complete circle.	S 3—50, P 3—50 } Full power. S 3—13, P 3—48 } Half power.	....	....	S 1—53 P 1—34 S 1—3 P 1—3
Diameter of circle in yards	155, 155 } Half power.	....	....	104, 92 } Full power. 94, 94 } Half power.
Diameter of propeller	5 ft. 10 in.	3 feet.	3 feet.	3 feet.
Number of blades	3	3	2	2
Immersed surface per ton displacement at 34 tons displacement	....	24.1	24.1	24.1

TABLE III.—*Shallow-draught river steamer.*

	October, 1882.	October 18, 1882.
Displacement ... tons	9.23	9.2
Slip ... per cent	44.1	....
$\frac{V^3 D^{\frac{2}{3}}}{\text{I. H. P.}}$	85.2	112
Revolutions per minute	480	....
Speed in knots	10.49	8.8
Diameter of propeller, No. 9_0 ... inches	16	16
Number of blades	2	2
Immersed surface per ton displacement at 9.8 tons displacement, square feet	61.3	61.3

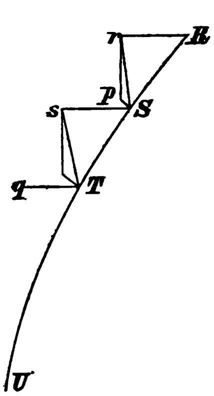
APPENDIX.

Note to explain Diagram No. 3.



A A A A is a section of the propeller, B B B is the boss. The mean path of the water which flows through the screw is along a surface of which C D is a section, any point c of which is determined by the equation $a\,e - ce^3 = ce^2 - be^2$, so that C D divides the volume swept out by the blades into two equal parts. The line C D is nearly straight, so let it be supposed to be straight. Then, to get a section of the blade along the mean path, a cone of wood C M N L is made such that its surface is a continuation of C D. Then, having fitted the cone and screw on the same axis, a sheet of paper is laid round the cone and is cut so as to fit the blade along C D.

Let R U be the section of the blade thus obtained. Draw Rr perpendicular to the direction of the shaft, and equal to the velocity of the point R round the shaft, and draw $\overline{r\,p}$=the velocity of the boat or of the water relatively to the boat. Then, while the blade travels from r to R, a particle of water will travel from r to p with the motion due to the original velocity, but as the particle must still be in front of the blade, the blade will have pushed it through a distance=p S in a direction perpendicular to R S. Thus $\overline{r\mathrm{S}}$ will be the velocity and direction of the particle at S. At S set off Ss equal to the velocity of the blade S, and draw $\overline{sq}$ equal and parallel to $\overline{r\mathrm{S}}$, and repeat the process as before till the end of the blade is reached. Let V be the velocity of the particle at the end of the blade; this will be the velocity relatively to the boat, and the slip will be V — $\overline{rp}$.



DISCUSSION.

Mr. W. H. WHITE. I am sure I am expressing the sentiments of all members of this Institution who have made any study of steam propulsion, when I say that we owe to Mr. Thornycroft the deepest debt of gratitude for giving us these results. To any one who has had to do with experiments at all, it must be obvious, as I have the pleasure of knowing by the personal facilities which Mr. Thornycroft has given me on various occasions, that there is an enormous amount of work and most patient research represented here in a form most convenient for other people to use, and Mr. Thornycroft has here put upon record information which I am sure will have an important bearing on the future of steam navigation at high speeds. That is my conviction. Having said that, may I ask Mr. Thornycroft if he will be good enough to give one or two other pieces of information which I think he has, but which I cannot find in the paper. I do not find in the paper—I may be wrong; I have only glanced through it casually—any clear statement of what increase in effective thrust with a given propeller Mr. Thornycroft considers to be gained by the use of the guide-blade and casing. I believe Mr. Thornycroft has that information, and it will be a most valuable addition, if I am right in thinking it is not here. Then I should like Mr. Thornycroft, if he can, to give us what the strength of the attachment for the guide-blade and its casings would need to be to prevent anything in the nature of rotary motion, if this system were applied on a large scale. It seems to me, if I apprehend the matter rightly, that so far as the guide-blades help the effect there would be that tendency, and it would have to be guarded against; that is, in getting the utilization of the longitudinal component of the thrust you have to deal with another component, and that would mean, I think, some considerable strengthening of the casings and their attachments with large engine power. Next, would Mr. Thornycroft add the slip-curve to this diagram? It would be such a complete comparison then with the late Mr. Froude's screw experiments. Mr. Froude gave curves of that kind, if I recollect rightly, connecting the efficiency with the speed of revolution and slip. Then with regard to the suggestion as to the possible performance of that shallow-draught steamer being better with a single screw than with two screws.

Mr. J. D'A. SAMUDA. Only in turning.

Mr. W. H. WHITE. In propelling. If I correctly gather the facts in Table III, when she had the two screws at work she was certainly being driven at a speed which was considerable in relation to her length. Is it not possible that with a vessel of that form when driven at speeds above 8 knots, you are getting beyond the limit at which the curve of power expressed in terms of speed-abscissæ would run up very rapidly? I don't know whether Mr. Thornycroft has that information, but I simply make the suggestion, and again thank him for his very valuable paper.

Mr. N. BARNABY. I was hoping, my lord, that some marine engineer might favor us with some observation, but as none has risen I shall be glad to be allowed to say how strongly I feel with Mr. White the benefit of having information of this kind placed so freely before us as Mr. Thornycroft has been good enough to place it. We have all of us heard of the things he has done with the Lightning and with that small vessel for the Congo, but we are not accustomed to having such early information given by people in his position enabling everybody to benefit by the experience he has gained by his careful calculations and experiments.

Mr. FRANK MARSHALL. My lord, as a marine engineer I am sure I only express the indebtedness that the whole of my profession owe to Mr. Thornycroft for this very valuable contribution to the proceedings of this institution. It is only one of the many debts of obligation which Mr. Thornycroft has laid us under in the various improvements that have come under our notice bearing on our profession. There is one difficulty I think we all labor under, if I might suggest it here, in connection with these papers, and that is the difficulty of our not having them in our hands before the meetings. I notice in the report of the council that it is the intention to publish these papers and to distribute them immediately after the sittings. Perhaps the council will take into their consideration the great utility it would be if we had the papers put into our hands before the meeting, in order that such subjects as Mr. Thornycroft has brought before us to-day might receive the attention that they so richly merit. It is quite impossible, coming to a meeting of this sort and hearing for the first time such problems enunciated as Mr. Thornycroft has brought before us to-day, to discuss them with that advantage which we might otherwise do. I strongly urge this upon the council. I again pay my tribute of thanks, and I am quite sure I only speak for my whole profession when I say that Mr. Thornycroft has in this, as in many other instances, added very valuable information to our stock.

Mr. J. I. THORNYCROFT. My lord and gentlemen, I have much pleasure in answering as far as I can the questions that Mr. White has put to me. I think the Table I I have put on the wall will assist him in some part. Mr. White asks what increase of thrust we could get, as I understand him, beyond what we are getting with the propeller now.

Mr. W. H. WHITE. Yes.

Mr. THORNYCROFT. In the table there is this column, "Thrust due to 12 inches diameter." That column makes all the propellers equal in diameter, and gives them working at their best efficiency; so that by comparing what you find in that column you can see how much more you can tax one of those propellers than you can tax an ordinary propeller and still get the best work. It is the fourth column from the right. There you will find that with an ordinary propeller of 12 inches diameter at 4½ knots you can have a thrust of about 12½ pounds. Then in examining the guide-blade propellers you will find on the same column the

first one is propeller No. 6_4. That propeller for its best work gives a thrust of 31 pounds instead of $12\frac{1}{2}$ pounds, at the same time the efficiency being about equal. There you have about two and a half times the thrust for the same diameter. What I had settled in my mind previously to writing this paper was that I could get twice the thrust for the same diameter, and that curiously enough (experiments seemed to prove that at first) amounts to this, that with an ordinary propeller you lose a certain amount (by an action similar to walking up more stairs than you get up), and also you lose a certain amount by friction. If you could have your stairs, instead of slipping down in that way, standing still at first, and only slipping towards the last, you could afford the stairs to slip away from you twice as fast and lose only just as much. That would lead you on theoretical grounds to expect that you could use just twice the thrust, and in designing the propeller for the Lightning we made that assumption—that we could use twice the thrust. In designing this shallow boat for the Congo it was very desirable to use smaller propellers than these, and in examining curves, one of which we have here, which represents a 9-inch propeller, there you see you can work to 15 pounds and get the best result. This is an 8-inch propeller. You can work up to nearly 30 pounds from that 8-inch propeller without any very great loss. Of course that may be said of a common propeller. You can work it without great loss beyond its best work, but in designing a vessel we must use our propeller to do its best work. Again, referring to this boat, there we had to use a very small propeller, and we had to get a very large thrust. We made the propeller with a very great increase of pitch and a very great contraction, but we found with a less contraction for obtaining a long pitch we got a very high thrust. Propeller No. 90, with two blades, when reduced to 12-inch diameter, gave a thrust for its best work of 32 pounds. Of course it is not certain that the propeller was put exactly to the amount of work it was intended for. The real facts of the case turned out to be these: When the boat was actually built it was found that one propeller would drag the boat and give a higher displacement co-efficient than it did with both propellers in use. I am not prepared to explain that.

Mr. W. H. White. I mean, if I may explain, that in estimating the displacement coefficient, it is assumed that the resistance is varying as the square of the velocity, whereas, as a matter of fact, it was probably varying from 9 knots to a higher speed in a greater ratio than that.

Mr. Thornycroft. I am afraid I am dwelling too much on one of Mr. White's first questions. I think possibly Mr. White is right in what he said about the lower speed of the boat, and that 12 knots an hour was beyond its best speed. That was also borne out by the fact, as will be seen in Table III, that at about 8 or 9 knots the displacement co-efficient is very much higher than at lower speed. And had the boat been intended for 8 or 9 knots instead of 12 miles the one propeller

would have been the best, but at 10 knots the one propeller still seemed to be the best. With regard to the attachment of the guide-blades to the vessel, it seems evident that if the guide-blades do what they are intended to do, if they take nearly all the rotary motion out of the water, again you will have exactly the same turning movement on your guide-blades as you have on your shaft, and the attachment will want to be just as strong as the fixings of the engine in the boat. Mr. White has asked also for something I am afraid I really cannot give him. He wants the slip-curve. There is one difficulty with this propeller in experimenting. The propeller was not all fast to the shaft, and this could not be attached without friction to some extent. In order to experiment with it, it was necessary to run it at such a pace that tube and guide-blades would propel itself. We have worked down as low as they would keep forward; when these would no longer keep forward we knew we were doing bad work, and we did not go below that, although it was quite easy to do so with a simple screw, as Mr. Froude had so well done, and really the method of that diagram is taken from Mr. Froude's. We were not able to continue this first line as Mr. Froude did down below there. There was also another reason why we did not do that. We found we were working our experiments at about what we thought was the best speed—we could not tell when we were doing that which was the best; it took us a day or two to get the results out—we had enough work to do to try the experiments where we knew there was something to be found, and not to be working on theoretical considerations. I must thank you very much for kindly hearing me at such length.

The President. Gentlemen, I am sure you will allow me to thank Mr. Thornycroft cordially on your behalf for his valuable paper. I should like to take this opportunity of making a few remarks upon a matter which has been brought before the meeting, because when you want a thing done there is nothing like hammering at it until you get it done. I have always found that in life. My friend, Mr. Marshall, has pressed a point which I certainly should have pressed if he had not raised it, because it is one of considerable importance to our members, that is, the getting of these very theoretical and difficult papers, raising extremely difficult problems, into the hands of members sooner. I am quite sure that our discussions would benefit very largely if that could be done. I have pressed it in former years, and I again press it now, and I am sure you will all agree with me on the extreme importance of having before the meetings papers in which problems of this difficulty are raised; and that they are difficult you can see from the fact that a man of the extreme ingenuity and ability of Mr. Thornycroft hesitates a little in answering the questions put by another extremely able man. That shows in itself that the ablest men are unable always to answer criticisms upon very difficult problems off-hand, before they have been able to work the thing out either by experiment or otherwise. I think when-

ever these papers are ready, and I would at once press this on our members, with great respect to them, that it would be for the benefit of our discussions, particularly when they are papers raising such extremely difficult problems, that they should be placed, if possible, in the hands of the members some days before they come up for discussion. The discussion loses by it if that is not so, because gentlemen naturally feel a modesty in getting up and dealing with matters of this difficulty at an extremely short notice. I pressed that before, and I press it again. I have only now, on your behalf, at the end of one of our longest sittings, I am bound to say, to thank our friend, Mr. Thornycroft, for his very admirable paper.

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Plate II

lers.

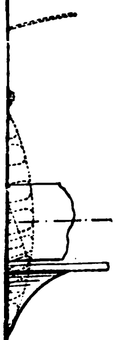
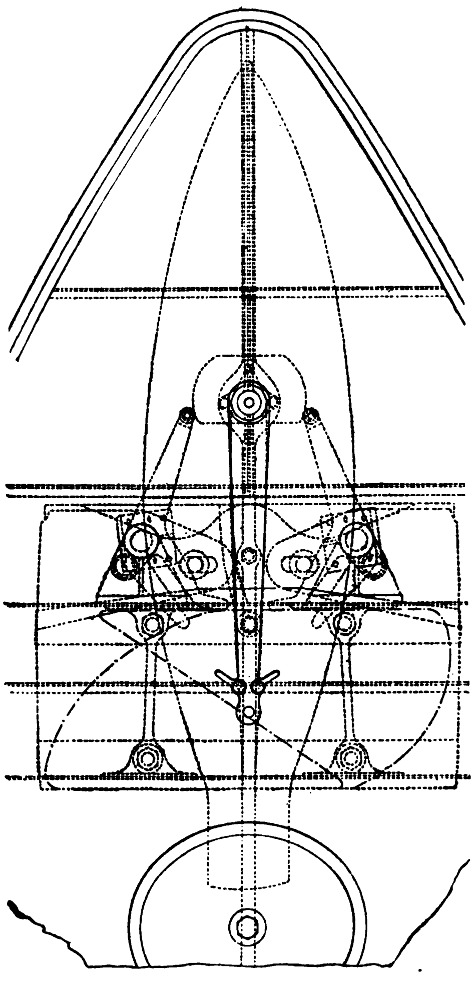


Fig. 3.



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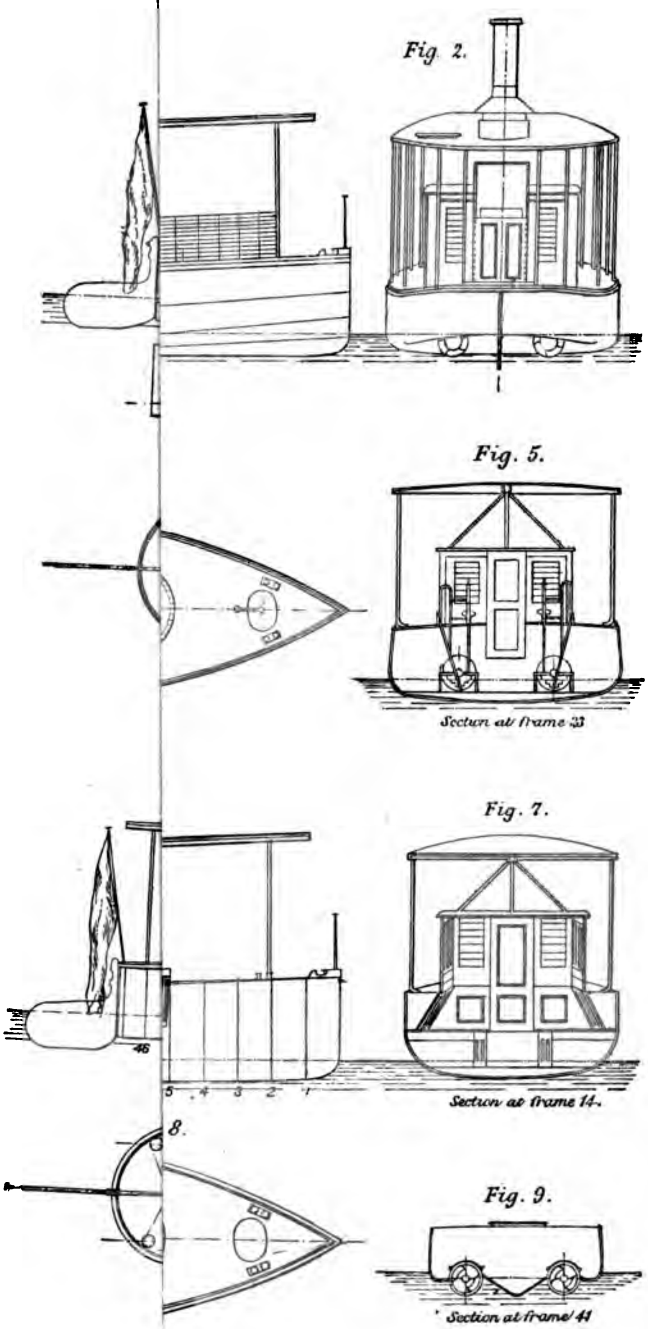


Fig. 2.
Fig. 5.
Section at frame 33
Fig. 7.
46
5 4 3 2 1
Section at frame 14.
8.
Fig. 9.
Section at frame 41

VIII.

ON SEA-GOING TORPEDO-BOATS.

By Mons. J. A. NORMAND, *Member.*

[Read at the twenty-fourth session of the Institution of Naval Architects, March 15, 1883; the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

By "sea-going torpedo-boats" we mean boats of from fifty to eighty tons displacement, having a maximum speed of eighteen to twenty knots, capable of steaming at least 1,000 nautical miles at ten or twelve knots, and of occasionally standing a gale, costing from £8,000 to £11,000, and manned by a crew of from ten to fifteen men.

It seems difficult to overrate the importance of vessels of this class in future warfare. Indeed, should their seaworthiness be proved, the subsequent consequences would follow:

(1) No iron-clad, no squadron or fleet, no cruisers (unless cruisers should attain the speed of torpedo-boats) could navigate in a sea of moderate dimensions, such as the Channel or the Black Sea, belonging simultaneously to powers at war, unless they should be escorted by sea-going torpedo boats, equal in strength to those of the enemy.

(2) Military ports situated in those seas or nearer than 200 or 300 miles to the enemy's shores would be rendered useless as stations for iron-clads or cruisers. For instance, supposing a war (which Heaven forbid!) between England and France, this would be the case for Cherbourg, Plymouth, Portsmouth, and Sheerness. Cherbourg and Plymouth could then be assimilated to two military ports whose entrances should be under the fire of each other, shot being here replaced by torpedo boats.

(3) Powers not having military ports sufficiently far from the enemy's shores, should be actually deprived of the use of their navy, with the exception of those vessels stationed in foreign neutral waters, unless they could force the blockade of sea-going torpedo-boats with a fleet of the same kind equal in strength.

As the fight between torpedo-boats must precede that between iron-clads or cruisers and torpedo-boats, the former must be armed accordingly with small guns, and especially revolving guns.

It is also possible that they ought to be escorted by one or several transports, carrying as many second-class torpedo-boats as possible, and serving as store-ships. In case of fine weather, the offensive power

of the torpedo-boat squadron should be increased, and the reprovisioning effected at sea. The escort would also greatly add to the security of the crews.

The above propositions are founded on the hypothesis that one squadron of sixty to eighty sea-going torpedo-boats, equal in men and cost to one iron-clad, are stronger than this iron-clad by daylight, and *a fortiori* at night even when reduced to half its number, the other half having left to coal and reprovision itself.

Could sea-going torpedo-boats be coaled at sea by means which are yet to be found, or could they, by such means as the use of liquid fuel, have their time of steaming doubled, their importance in warfare would be immensely increased.

No second-class torpedo-boats could replace boats of this kind, because they cannot stand a gale, nor can they be then lowered or shipped, so that the enemy can escape the attack of small torpedo-boats by taking advantage of bad weather.

The question now is, are torpedo-boats of such small displacement as from fifty to eighty tons really sea-going? If they are not, can they be made so? Time and experience will show, but we already know that with their steel deck and hatchway coverings they can stand very bad weather.

One thing, however, is certain. The use of the torpedo is rapidly gaining a wider range. Before long we may see it replacing the ordinary ram, that most barbarous of all weapons for ships navigating in squadron, and which has already done more harm to friends than to foes. Should this substitution take place, the ram would remain, but it would be innocuous or offensive, at will. Of the three weapons in use at sea, gun, torpedo, and ram, the first alone can be guarded against by armor, and that by the sacrifice of money, men, speed, and handiness. As regards the two others, all ships are equally vulnerable. Should torpedo science continue to progress as it has already done, it will be nearly usless to protect ships at such fearful expense against shot, leaving the immersed portion unprotected, and the time may come when fleets will consist of cruisers and torpedo-boats or vessels.

APPENDIX.

It may be interesting here to note the results of the official trials, made last summer at Cherbourg, of the sea-going torpedo-boat, No. 60, the first of a series built or building by the author for the French Government.

HULL.

	Feet.	Inches
Length at load line	108	2
Breadth, extreme	18	10

ENGINES.

Diameter H. P. cylinder	inches..	12.60
Diameter L. P. cylinder	do....	20.48
Stroke	do....	14.96
Heating surface (fire-side)	square feet..	816.00
Grate surface	do..	19.3

TRIALS.

I.—*Full speed, three hours' trial.*

The boat was complete, with launching-tubes, compressing-engine, air-reservoir, six 19 feet Whitehead torpedoes, 2½ tons coal, two Berthon collapsible boats, no masts.

Displacement	tons..	43.00
Mean speed	knots..	20.62
Consumption of coal during the three hours	tons..	1.58
Consumption of coal per hour	do....	.53

(We believe this low rate of consumption, taking into account the speed and the weights on board, to be unparalleled.)

Indicated H. P. (about)		500.0
Revolutions per minute		328.5
Boiler pressure	pounds..	132.0
Air pressure	inches..	3⅝

2.—*Low-speed trial.*

Same weights as above, but with 5 tons more coal, and about half a ton provisions.

Displacement	tons..	48½

The trial lasted forty-eight hours *without stopping*, from August 19, 8 o'clock in the morning, to August 21, same hour.

Weather fine for the first eight hours, rough for the forty remaining hours.

Mean speed	knots..	12.70
Total consumption of coal	tons..	5.91

Although the engines raced frequently, all went well throughout the trial.

The boiler was not coated during the trials.

Both trials were preceded by runs on the measured mile.

DISCUSSION.

Mr. YARROW. My lord and gentlemen, I am sure I express the feeling of the meeting in saying that we are greatly obliged to M. Normand for laying before us his views on this subject. It shows a very kind feeling on the part of so distinguished a French gentleman to come forward and prepare a paper for an English institution, as he has done on this occasion, and it would be a good thing to have more interchange of opinion between engineers of different countries, and so long as it is not a one-sided interchange it will be beneficial to both parties. Consequently, in order to reciprocate what M. Normand has initiated, so far as torpedo-boats are concerned, I would intimate to the distinguished French gentlemen here that we shall only be too pleased, when a favorable opportunity occurs, to prepare a paper for some corresponding institution in France. I am almost afraid, gentlemen, that the boat-race

is more interesting than any remarks I would make. Therefore, as regards anything I might say, I would suggest it be sent to the secretary later on for publication with the transactions.

The PRESIDENT. That is not in accordance with the rules; so that anything you have got to say, in spite of the boat-race, please say it.

Mr. YARROW. M. Normand mentions that the consumption of coal on his full-speed trial is unparalleled, and I take this opportunity of confirming that. I am only sorry that M. Normand is not here, so that we might ask him some questions as to how he arrived at it. As regards sea-going torpedo-boats generally, what is meant by that expression really determines the design of the vessel, and it rests with each individual Government to say what those requirements are. I take it that the two essential conditions of a sea-going torpedo-boat are that it shall have a speed, fully equipped, of not less than 19 knots, as anything less than 19 knots under those conditions would, I submit, at the present time not be up to recognized requirements; and, secondly, that a boat of this class should be of as small dimensions as possible, with a given fighting power, that is to say, of two boats having different dimensions, but the same speed and the same fighting power, the smaller one is decidedly the preferable one of the two, because it exposes a smaller area to the shots of the enemy's guns. A sea-going torpedo-boat must clearly be capable of going out to sea in any weather. If it is required for the defense of a port and the adjoining coast, I would submit that one of 100 feet long, by about 12 feet 6 inches beam, gives thorough sea-going qualities, which has been proved by the number of voyages boats of these dimensions have made; they have sufficient coal-carrying capacity and accommodation to conform to the necessary conditions for the defense of a port and adjoining coast, and this may be obtained at a cost of from £8,000 to £9,000. If the sea-going boat is to make long voyages under steam, say 2,000 miles, and with suitable accommodation, I would submit that nothing less than 125 feet or 130 feet in length would meet the requirements, and the cost of such a vessel would be about £12,000. Then comes the question, how far it is well for so large and valuable a vessel to be unprotected, and the question at once arises, how far armor protection can be secured, and on what conditions. I believe it is possible to design a vessel 160 feet in length having all the machinery and torpedo gear encased at the sides with 1½ inches steel, and deck with 1 inch steel, at a cost of about £28,000, having a speed of 19 knots. I introduce the cost because it would seem to me that, having a given sum at their disposal, it rests with naval authorities to say what is the best way of spending that sum. I would say now a few words touching the desirability of having twin screws for torpedo-boats. Mr. White, in his excellent work, has clearly pointed out the superior performances of twin over single screws in ordinary vessels, due, no doubt, as he says, to the more advantageous position of the propellers compared with that of the single propeller, as

usually adopted. But I do not think, considering the position of the single screw in a torpedo-boat, he would expect any advantage in performance by adopting the twin screws. There is one point, which I think an important one, in connection with twin screws for torpedo-boats to which I would draw attention. If it is desired to attack a vessel end on and retire end on, so as to expose as small an area as possible to the enemy's fire, with a single screw vessel, it is well known that, on the boat being stopped and the engines started astern, it will not go back in a direct line, but will turn until sufficient way is secured for the rudders to obtain command, thereby presenting the entire side of the torpedo-boat to the enemy's fire, and that at a time when it is almost at a state of rest. If, on the other hand, twin screws are adopted, the boat would be under entire command from the very moment of going astern, and I believe it is the opinion of naval authorities that, if that could be attained, attacking end on and retiring end on would be a very desirable mode of attack. I would mention that the importance of this point was first pointed out to me by Count Hoyos, of the firm of Whitehead & Co.

Mr. J. D'A. Samuda. My lord and gentlemen, it seems to me that we are almost as much indebted to Mr. Yarrow for the explanation which he has given as we are to M. Normand for the paper which he has presented us with; but what I would desire to call attention to is this: That this paper is a much more ambitious paper in its design with reference to the way in which it proposes to use vessels than Mr. Yarrow has treated it as being. From the point of view in which Mr. Yarrow has it treated, that is to say, as a torpedo-boat, it is very proper to use exactly the arguments that he has used, but what we have to look at with reference to this paper appears to me to be this: Is the author of this paper right in supposing, as this paper evidently does suppose, that by the introduction of such torpedo boats as he describes here, viz, torpedo-boats of 50 to 80 tons' displacement, we should be able ultimately (to use his own words) to reduce the contest which will have to take place by saying that "the time may come when fleets will consist of cruisers and torpedo-boats only." Now, I think you must therefore take this paper in the interest of the writer of it with regard to the possibilities of giving effect to his ideas by the means which he puts before us. I venture to think myself that he is entirely wrong in his view. I venture to think that no boat whatever of these sizes would be suitable for the services which he designs to apply it for. I quite agree as to the enormous value that torpedo-service is about to render, though probably it has not developed itself sufficiently yet to say it has done that which many of its advocates desire to say for it. But it certainly will go on improving, and will do it. Therefore our right way is to treat it as though it had accomplished the purpose which it is making such very rapid steps at the present moment towards accomplishing. That being so, I agree with so much of this paper as says

(and I believe we must all of us agree to this) that it is not desirable to leave our large iron-clad vessels which are building at the present moment entirely dependent upon their own means of defense against torpedo-boats that may be sent against them. Neither would it be desirable to limit their means of defense to torpedo-boats such as they might carry on board their vessels and in their davits. I believe, too, that you will find it an absolute advantage, if not a necessity, to take with the fleet scouts, in the shape of one, or two, or three torpedo-boats, accompanying every one of these large iron-clads, boats which will be capable of keeping the sea and maneuvering with it. That is what we have to look to; and then the question is, whether boats of this size and description at all approach to the work you require to do. I think not. I think if you would be content to avoid all armor protection, or a very large portion of it, that such vessels, vessels of the sizes that were spoken of yesterday at the time of Captain Noel's paper being read, would be very much nearer the description required. These would be vessels of some 1,200 tons, instead of being 50 to 80 tons' displacement. These would be vessels that would be able to carry large guns; guns which could penetrate small armor-clads, or attack cruisers unarmored at a considerable distance, and would be a most valuable accompaniment for the fleet, something like cavalry to an army, or skirmishers to an infantry regiment on going into action, or in protecting the iron-clad when taking up its position, or in going into a general engagement with the iron-clad; but vessels of the small size suggested by M. Normand are wholly inapplicable, and I am not by any means certain, if you could afford the money, that even then you would do well to confine it to such vessels as were spoken of yesterday. I am not quite sure that money should be an absolute *estoppel* to you; and it ought not to be, because in these matters money ought never to be considered. The sole condition which I believe you ought to have in view is to accomplish your object, no matter at what cost, because I have always held—and I think the thing cannot be too prominently put forward—that to fail in what a nation desires to do in case of war must inevitably entail upon it enormously greater expenses than those which would have been necessary to have enabled it to succeed. I think so, and when we look at what has taken place in modern history—in the result of the late war, for instance, between France and Germany—we see that such great fines are imposed upon the unsuccessful party that it is quite impossible safely to neglect anything that you can conceive will most effectually carry out the end which you have in view. Going, then, farther on in that direction, that is to say, to getting the most complete mode of defending a great iron-clad before that iron-clad shall be brought into contact with vessels like itself or with any other fleets which it might have to encounter, or with any other vessels which it might have to meet in much greater numbers, but not armored like itself, even the protection of armor would be of great assistance to

these auxiliary vessels that would be accompanying the fleet, and, if you desire to give them a moderate amount of armor, then 1,200 tons would not be sufficient, but you would have in all probability to double the sizes of these vessels. Still, I think myself that it is not at all necessary, in arguing this paper, that one should take a different line of departure from that which is accepted as sufficient by the writer himself, viz, that a vessel of the same description as that which might destroy an iron-clad should be the means of opposing and preventing its destruction. That could be very well done without vessels plated with armor; and then I think that with vessels of something like 1,200 tons' displacement you might carry such powerful guns, you might also carry torpedoes, that no torpedo-boat could at all approach or come within anything like an attacking distance of your large iron-clad, and you would by this means be able to keep off this great attacking force which M. Normand fears might ruin the iron-clad and destroy it. As to the idea of being able to have a vessel of 50 tons that should go out and accompany the fleet, it appears to me that such a vessel would be utterly incapable of doing the service that is required of it, that is to say, beyond being a mere torpedo-boat itself.

Mr. R. H. ANDREWS. My lord, opinions differ as to what is the best size for a torpedo-boat. Some men advocate large ones, and some small ones. No doubt both kinds are useful in their way. My opinion is, that small torpedo-boats, like the second class in our navy, are useful under a few conditions only, and principally as picket boats, to give warning of, and keep off boats of the enemy. I have no hesitation in saying that they can only be used as torpedo-boats in fair weather, and that it will be a matter of impossibility to fire the torpedoes from them with any degree of accuracy if there is any sea on. If they were fired, it would be quite as reasonable to expect that they would hit themselves, or their friends, as the enemy. The larger the boats are the steadier they will be in rough weather, and, consequently, will give a better chance of striking an enemy's ship with their torpedoes than the small boats will. I think there should be large torpedo-boats to accompany a fleet, so that torpedoes may be fired against the enemy in bad weather, and also small torpedo-boats carried in ships to be used as torpedo-boats in fine weather, and as picket boats to keep off night attacks by the enemy's boats in all weathers. I have no doubt that in any future naval war torpedo-boats will principally be used by night, and that it will be desirable to have a large number of these boats in the fleet, so as to form a cordon of one or two lines of boats around your own ships to keep off the attacks of the enemy's boats. I suppose no one will deny that the greater the number of boats, whether torpedo or ordinary boats, the better will be the protection to their ships. I have been told that during the Russo-Turkish war Admiral Hornby had our fleet anchored in a bay in the Black Sea, the entrance to which was very narrow, and as an experiment he gave orders for a

few ships' boats to go outside the entrance of the bay, and during the night the officers in them were to try to steam the boats into the bay and up to or near the ships. Sentries were placed on each side of the entrance, the electric light was used from the ships, and picket boats were steaming about, and yet some of these attacking boats passed through the entrance and got within torpedo attacking distance of some of the ships without being discovered. It seems to me, therefore, that a great number of small torpedo-boats will be very useful indeed, and some large ones with a fleet will be indispensable, for the reasons I have given. The question as to the proportion of large to small torpedo-boats in a fleet is one which cannot easily be answered, as the cost must be considered as well as other matters. If we suppose a large first-class torpedo-boat to cost £10,000, we can get four second-class boats for the same money; but it is very difficult to say definitely which would be the most advantageous way of laying out this money, the purchase of one large boat or of four small ones. Referring to the results of the full-speed trials, given at the end of the paper, I think that, as M. Normand has been so generous in preparing this paper to be read at these meetings, we might ask him if he will kindly attach to it, to be printed in the Transactions, a drawing of the boiler of the boat in which the trials were made.* My reason for asking this favor is, that the consumption of fuel on the three hours' full-speed trial is, I think, unprecedented for a torpedo-boat; certainly it is in my experience. When we find a torpedo-boat of 43 tons' displacement being driven over 20 knots per hour on a consumption of fuel of 61.5 pounds per square foot of grate per hour, or 2.37 pounds per i. h. p. per hour, we cannot but call it a very economical result, and one that has not as yet been attained in any of Messrs. Thornycroft's or Messrs. Yarrow's boats, under the same conditions. It is practically impossible to make more economical engines than those by these two firms; consequently, the boiler must be a better one for steaming than those usually fitted in our torpedo-boats, and therefore a drawing of it would be deeply interesting to us all and would be a very valuable addition to M. Normand's paper, and I trust that he will pardon me for making such a request.

Vice-Admiral De Horsey. My lord, I wish to add my opinion to that of Mr. Samuda, that a torpedo-boat of 50 tons cannot be qualified to keep the sea. We read in the high-speed experiment of M. Normand that she had on board 2½ tons of coal; that she performed her great speed of 20.62 knots very economically, but that during three hours she burnt 1.58 tons. Therefore she could last for something under six hours. Now, I would ask the meeting what use would be a sea-going torpedo-boat whose powers were limited to six hours? She might be six hours waiting for the enemy, or sixteen, or sixty. I think, my lord, that the truth of the matter as regards torpedo craft is this, that we must have

* See letter from M. Normand at the end of the discussion.

different sizes for different work. The vessel here described, the 50-ton torpedo-boat, would, I have no doubt, be admirable for issuing from our ports to attack an enemy. When you come to Channel work you want a size larger; when you come to Mediterranean work you must have a size yet larger, and when you come to ocean work, there, I think, torpedo *boats* (except those capable of being carried at davits) stop. You may have huge torpedo vessels at enormous expense, and in my opinion there is no reason whatever why we should not build torpedo vessels to go 30 or 40 knots. I believe it is possible; but none exist, and I think this is beyond the scope of the torpedo *boat*. I believe we shall not find it practicable to have attached to the fleet independent sea-going torpedo-boats of small size. They will not be able to take care of themselves. If they should expend their coal (and it is reasonable to suppose that that might happen) they would be totally unfit to encounter a gale. Without coal they would lie broadside to the sea; they would be rolled over in all probability by the first very heavy sea that struck them. There was one point to which Mr. Yarrow adverted, as to the advantage of attacking end on and retiring end on, and he alluded to single screws and twin screws, and said, what is very well known, that with the single screw you cannot go astern without losing your line of direction and turning to one side or the other. Well, gentlemen, the only plan that I have seen suitable for torpedo boats is that of Mr. John Samuel White's, that of cutting away the dead-wood. You are very likely familiar with that plan, which includes the addition of a balanced rudder *before* the propeller. You can steer the boat going astern just as well as an ordinary boat going ahead. Her powers of turning are marvelous. Dead-wood, as you are all aware, is a contrivance for making sailing vessels weatherly. The moment we come to steamers, dead-wood is immaterial. Those who have not seen experiments with boats without dead-wood would be astonished at their turning powers and steering power going astern, elements which are, above all, requisite for torpedo-boats.

Mr. H. MORGAN. My lord, I had had an idea of rising in the early part of this discussion to say that I thought M. Normand was a little too sanguine—I may say a good deal too sanguine—as to the possible power of these torpedo-boats, but my friend Mr. Samuda got up and said that as well as I could say it, and I may say he said it so very well that now I am going altogether on the opposite tack. I think, on the other hand, that Mr. Samuda has rather underrated, perhaps, the possible performances of boats of this sort, and he has been followed in that idea by Vice-Admiral de Horsey, both of whom are admirable judges of what might be done. In the outset I would make this little correction. Mr. Samuda and Vice-Admiral de Horsey spoke of boats of 50 tons' displacement. M. Normand speaks of boats of from 50 to 80 tons. We must allow him the credit of his upper limit—80 tons' displacement—which would probably correspond to a boat some 130 feet long. I have had to

think a great deal about the possible uses of such boats, and am responsible for having recommended the building of such boats. I am sorry we have not reached the point of having such boats in the English navy. While, on the other hand, in support of the view I am putting forward, I may quote the fact that such boats are possessed by almost every other country, and I believe I am right in supposing that they are possessed by those countries with the idea that they would do certain work at sea. I may now refer to an event of which we are all very proud, and which has been referred to you by your lordship and other speakers, viz, the bombardment of the forts of Alexandria. If the Egyptians had possessed a few torpedo-boats, that bombardment could not have taken place. You may say it could have been prevented by small torpedo-boats. So it could by boats like our second-class boats; but suppose the Egyptians had possessed boats of 50 to 80 tons' displacement, which could have followed our fleet some miles to sea, and which need not have cared about encountering some amount of rough weather, and could have remained at sea some time, I am sure in that case our fleet would have found it rather difficult to remain off Alexandria at all. Take another illustration. Suppose that an enemy's fleet were coming across the Channel in tolerably fine weather to attack Portsmouth—or rather, what would first happen, of course, to attack our fleet. If that fleet were accompanied by twenty or thirty or fifty, or you may say one hundred, boats of 50 or 80 tons' displacement, then, I say, the power of that fleet would be most enormously increased against the power of our fleet, supposing our fleet had not those vessels. I am speaking now, of course, of possible occurrences in the English Channel and in moderate weather. I know, as Vice-Admiral de Horsey says, that for different services you want different sizes of boats. We have our little second-class boats, which are carried in ships, and which can be put out whenever there is a chance, whenever the weather and other circumstances are favorable, which could operate, for instance, in any inland waters, the Black Sea, and so on, and which could follow an enemy's ships into their own ports if necessary. Then we have our first-class boats, which are intended to operate from the port—which might defend Portsmouth, Liverpool, Malta, Gibraltar, or any other such places as those. But I do think if you go a little further and improve the boats in the way they have been improved, in part by Mr. Yarrow and Mr. Thornycroft, that such boats would be found to be very powerful in co-operation with the fleet. Vice-Admiral de Horsey mentioned that these boats could only steam for six hours; but that is at full speed. These boats can go 1,000 knots at half speed. If boats were hovering about the Channel or anywhere else for an enemy's fleet, they could remain a very long time, because they would not want to steam very fast, and they could just go fast enough to take care of themselves in the sea. Vice-Admiral de Horsey has referred to the very excellent plan recently introduced by Mr. John Samuel White. I have the pleas-

ure of knowing something about that plan, having been consulted by Mr. White about it from the beginning, and I have been in several of Mr. White's boats. I will not say any more about it, because Mr. White, while he could not be brought up to the scratch to read a paper here this time, will, I hope, read one next time. I will only say, in conclusion, that that plan adds very much indeed to the power of torpedo boats, and very likely it will be applied to larger ships as well.

Mr. W. H. WHITE. My lord, I do not propose entering into discussions on the general policy of naval construction, but I think there are two passages in M. Normand's paper which, in justice to him, ought to be kept in view. I suppose we are all agreed that vessels of various sizes are wanted for different purposes, and Mr. Samuda would not despise these large torpedo boats. If they were in possession of the country there would be a use for them.

Mr. SAMUDA. Undoubtedly.

Mr. W. H. WHITE. Mr. Samuda's view, as I understand it, is that we should, as a matter of policy in spending, not stand at the money, but build vessels that could do this work and more. So far, I think, there can be no question about it; but M. Normand evidently had chiefly in view the use of this type of vessel within narrow limits of service. He did not quite conceive their going into the middle of the ocean, and waiting for an indefinite time to intercept a fleet belonging to the enemy. His examples are chiefly chosen, you will see, from service in narrow waters and inclosed seas. As a matter of fact (Mr. Yarrow will correct me if I am wrong), I believe that with the rig of which we see an example in the model on the table these boats are almost self-supporting under sail.

Mr. YARROW. Yes.

Mr. W. H. WHITE. So that the difficulty which Vice-Admiral de Horsey felt as to the use of coal when waiting is one which could be got over. Another point I should like to refer to is this: These boats are practically unsinkable and uncapsizable, and the variation in stability produced by burning coal to which Vice-Admiral de Horsey referred might, of course, be got over, supposing them to be capable of proceeding under sail, by water ballast. These are points of detail, but they must be considered in order to do justice, as I know we shall all wish to do, to M. Normand's advocacy of this particular type as adapted for certain uses. The second matter to which I should like to draw attention is at the bottom of the first page in this paper. No allusion has been made to it, but I think it is of the highest importance. M. Normand says, in the last sentence, that the torpedo-boat "must be armed with small guns, and especially revolving guns." I think that is the key of the whole position. These boats may be successful against large ships. That is conceivable. They have not been so yet when they have been tried, but they may get a lucky slant and succeed; but they will be kept off, if they are kept off at all, by similar boats; that is to say,

a torpedo-boat ought to have a powerful machine-gun armament to meet other torpedo boats. To fire a Whitehead torpedo at a rival torpedo-boat would be rather a risky and certainly an expensive way of destroying a troublesome foe. If M. Normand were here, I feel confident he would be very happy to give the information justifying his claim to economy of coal consumption. The figures are, as Mr. Yarrow says, very striking, and I think when he knows—I will not say the doubt, but the hesitation that has been felt about them—he will be very happy to try and justify them as they stand.

The PRESIDENT. Gentlemen, it only remains, as nobody else rises, for me on your behalf to convey to M. Normand our thanks for his paper, and our great regret at his absence and his consequent inability to be here to explain his paper. You observe, gentlemen, the enormous value of your institution in reference to this and similar papers furnished to us by our foreign members as a means of intercommunication of ideas, not only here in England but in all foreign countries. I do not think we can appreciate too highly the value of that intercommunication of ideas. They have a good deal to tell us; we have possibly a good deal to tell them. Were it not for this institution I do not know what means there would be of that ready intercommunication of ideas, without rivalry, and in tones of perfect friendship, merely carried on in the interests of science and progress in ship-building. I think you will agree with me that you should look at this question as highly in that light as I do. I am sure you will allow me, gentlemen, to convey to M. Normand our thanks for his very admirable paper.

The following letter relating to the discussion on his paper has been received from M. Normand:

HAVRE, *June* 5, 1883.

DEAR SIR: In answer to your letter of the 1st instant, I regret that I do not feel at liberty, without the sanction of the French Government, to publish the drawings of the boiler of No. 60 torpedo boat.

I have heard from friends of mine that the accuracy of the economical performance of the boat was challenged at the meeting, which I regret exceedingly having been unable to attend. The data given in my paper were taken from the *official* report, and Messrs. Thornycroft and Yarrow know themselves with what care these reports are drawn.

I may add that the relatively very small consumption of coals in my torpedo boats is not, in my opinion, exclusively due to the boilers.

I remain, dear sir, yours, very sincerely,

J. A. NORMAND.

GEORGE HOLMES, Esq.,

Secretary to the Institution of Naval Architects London.

IX.

SOME EXPERIMENTS TO TEST THE RESISTANCE OF A FIRST-CLASS TORPEDO-BOAT.

By A. F. Yarrow, Esq., *Member*.

[Read at the twenty-fourth session of the Institution of Naval Architects, March 15, 1883; the Right Hon. the Earl of Ravensworth, president, in the chair.]

The object of the present paper is to give a brief description of some experiments which we recently tried, to ascertain in a first-class torpedo-boat, propelled by various speeds, the indicated power of the engines, and the thrust on the shaft when steaming in the usual way; also the power required to tow the boat.

The boat upon which the experiments were tried was 100 feet in length, by 12 feet 6 inch beam, having a displacement of 40 tons, and for the purpose of towing it at as high a speed as possible we used another torpedo-boat somewhat larger, of about 50 tons' displacement.

The first set of experiments were to ascertain the indicated power for various speeds. The indicators used were of Mr. Darke's latest pattern. The engines were of the ordinary direct-acting inverted surface-condensing type, having cylinders 12½ inches and 21½ inches by 16-inch stroke, capable of making, at 120 pounds per square inch, 480 revolutions per minute, giving a speed of twenty-two and a half knots.

The curve obtained is shown by the line "I" Diagram A (Plate I), which has been arrived at by numerous observations.

How far diagrams obtained in the usual way with exceptionally high-speed engines may be relied on to represent exactly what passes in the cylinders, is no doubt a matter for consideration.

The next set of experiments was to ascertain the thrust on the shaft at various speeds. For this purpose we designed a dynamometer, which is simply a modification of Mr. Duckham's weighing machine. Its exact nature will be clear from Diagrams B (Plate I). There are two hydraulic rams of a diameter giving, collectively, exactly 6 square inches. The two ram cylinders were securely bolted to the bed-plate of the engine, one on each side of the aftermost main bearing, and the rams were secured to the thrust block, which is generally bolted down to a bearer in the boat, but which in this instance was left quite loose, and allowed to move freely in a forward and aft direction. It was prevented from revolving by means of two stays about 7 feet long, secured at their

upper ends to the deck. It will be clearly seen that, with this arrangement, immediately there is a thrust on the shaft the thrust block would move bodily forward, pressing the rams into the cylinders. Copper pipes were led from these cylinders to pumps and to pressure gauges fixed in the after cabin, where they could be conveniently observed. As the rams were exactly 6 square inches in area, and as we could read off on the gauges the exact pressure per square inch, it was a very simple matter to arrive at the total amount of the thrust.

We had three pressure gauges, for the sake of comparison, and we used three pumps, two of them being ordinary vertical reciprocating hand pumps, and the other a ram worked by means of a screw. The vertical hand pumps answered the purpose very well for pumping the ram cylinders full of oil in the first instance, but owing to their intermittent action they were unsuitable for use while any records were being taken.

The pump having its ram worked by a screw we found, by its steady and uniform motion to answer well, giving us the utmost pressure required without difficulty. On first testing the apparatus we found the pressure gauges vibrated very considerably, too much so for a correct reading to be obtained, which we believe was due to the unequal strains on the shaft during each revolution. To obviate this, a large air vessel was placed on the pipe uniting the rams with the pressure gauges. The greatest thrust on the shaft obtained was 4,080 pounds at 15.735 knots.

The coupling between the thrust shaft and the crank shafts was of the usual type, steel bolts being secured in the one coupling, passing loosely through holes bored out in the other. Consequently, when the thrust came on the shaft and it moved forward, the amount of pressure registered on the rams would be equal to the thrust on the shaft, minus the friction due to these bolts sliding through the holes and the friction of the rams themselves. Then by working the pump and forcing the stern shaft in a sternward direction, the pressure obtained on the rams would be equal to the thrust plus the friction on the bolts and the rams.

We took the mean between these as representing the thrust, and to be as accurate as possible several pairs of these observations were made at each speed, and the mean of them taken as a fair estimate of the actual thrust. The corresponding horse-powers obtained by multiplying the thrust into the speed of the boat divided by 33,000 gives the curve shown by the line' 2."

The shaft not being quite horizontal, the thrust obtained in this way clearly does not correctly represent the horizontal pressure. Resolving the inclined thrust in a vertical and horizontal direction, we find the difference between the total pressure in the direction of the shaft and the horizontal pressures—*i. e.*, for purposes of propulsion the loss due to the inclinations—as represented by a quarter to three-eighths per cent. only for speeds within the limits under consideration.

We next proceeded to tow the boat upon which these experiments were

tried by another boat, as explained. For this purpose we used the same dynamometer, modified to suit, and adopted the same system of obtaining the pull as we had previously in obtaining the thrust, so that any errors due to the dynamometer itself should be common to both sets of experiments. The exact arrangement of the gear is shown by Diagram C (Plate I). The dynamometer was secured on the deck of the hauled boat, the rope passing direct from it over a pulley at the bow to the towing boat. The towing experiments were limited to 14.97 knots, which was the utmost speed we could maintain continuously.

The length of the tow line we used varied, the mean length being 450 feet, which we believed to be sufficient to avoid the sternward column of water produced by the screw of the towing boat from being perceptibly felt by the towed boat.

At first we found great difficulty in getting anything reliable, as the wave due to the towing boat materially influenced the result. To meet this we tried a number of experiments at as nearly as possible the same speed, varying the lengths of tow-line, so that in some cases the waves formed should retard, and in others help, the boat, taking the mean of them to represent fairly the correct result, which is shown by the line "3".

In these last experiments the screw of the towed boat was removed.

I would mention that, through the courtesy of Mr. Barnaby, some model experiments were carried out by Mr. Froude upon a very similar boat, having the same displacement as the one upon which we made the foregoing trials. These experiments extended from speeds corresponding to 11.7 up to 23.5 knots.

Although we do not presume that our experiments were conducted with the same care and accuracy as done by Mr. Froude, still it is interesting to note that at those speeds common to both Mr. Froude's trials and ours, Mr. Froude's resistances were less than ours by about 3 per cent.

As the form of the stern of these boats and the position of the propeller with reference to the stern are elements greatly influencing the results obtained, I thought a model would better explain these than any design, and therefore beg reference to the model on the table for illustrating these points.

DISCUSSION.

Mr. R. E. FROUDE. My lord, I should be sorry to let this paper pass without saying a few words in recognition of the extreme importance of these experiments of Mr. Yarrow with reference to the inquiry which the Admiralty are conducting at Torquay under my superintendence. The work which we have there to do is to test the resistances of a variety of models, and from those resistances to calculate as best we

can what will be the resistances of the corresponding ships. The laws on which that calculation is based rest on an extremely strong theoretical foundation as regards the main elements of resistance, but there are undoubtedly some elements of resistance as to which we feel some uncertainty, and therefore any experiments on the actual resistances of full-sized ships, even of full-sized torpedo-boats, are of extreme value. Such experiments were made, as most of the members of this institution will remember, on the resistance of the Greyhound and experiments on the model of the Greyhound were also made at the establishment at Torquay, and the results yielded by the model calculations and those of the actual ship agreed very well. But the phenomenon of a boat proceeding through the water at such a very high speed compared to its length as a torpedo-boat is of an exceptional character. It is a totally different kind of operation to the phenomenon attending the passage of a vessel through the water at the speeds which full-sized ships attain. Therefore, an experiment on the resistance of an actual torpedo-boat is of extreme value, and the more so because the results of the experiments which we have made on models of torpedo-boats assign resistances to the full-sized torpedo boats, which were certainly large compared with the indicated horse-power which is given by trials on the boats themselves. The theoretical horse-power due to the resistance of the boat multiplied into the speed, as calculated from our model experiments, compared to the indicated horse-power on the trial, does not allow margin enough for the inevitable losses in the action of the screw and engines. Therefore it seems either that our resistances, as calculated for the full-sized torpedo-boats, must be in excess, or that the indicated horse-power is underrated, which I certainly think is possible, considering the enormous number of revolutions per minute at which the engines run. Mr. Yarrow states that the actual resistance of the torpedo-boat that he tried was 3 per cent. greater than the resistance calculated from the model experiment; therefore that sets my mind quite at ease upon that point. I think it is evident that that is an exceedingly good agreement; I should have hardly expected that we should be able to calculate the absolute resistances as correctly as that. I have only just had an opportunity of seeing the paper, and therefore I have not been able to give sufficient consideration to the method of experiments which Mr. Yarrow has adopted to be in a position to criticise it, but as far I can see it is extremely ingenious and perfectly well adapted to the purpose. I am very glad in particular to see that Mr. Yarrow has "spotted" (if I may use such a word) the difficulty attendant on the waves following the towing boat. It has been proposed by the Admiralty to make some experiments on a torpedo-boat of this kind, though we have not had an opportunity of doing it yet, and in considering how the experiments were to be made the very same difficulty had occurred to me, and I intended to meet it in the

same way as Mr. Yarrow has done, by trying experiments at the same speeds with various lengths of tow-rope.

Sir EDWARD REED. My lord, I should like to ask Mr. Yarrow what determines the point of connection of the tow-rope with the vessel. We all know that in towing, the manner in which you take hold of the towed ship is of very great importance as regards resistance, and I should like to ascertain how he knows, when taking hold of the vessel, that the vessel would be towed in proper trim and would offer the normal resistance. I suppose he took steps, perhaps by tentative experiment, to ascertain where to take hold of her, but I should like to know whether he did, and how it was that he fixed upon the spot that he chose for making the attachment, and whether he made any observations for the purpose of showing what trim the vessel was being towed in as compared with the trim and other conditions when she was being steamed with her own power. That is the only point that occurs to me as requiring some additional information, and that only for the purpose of illustrating the more or less nearness of the theoretical with the practical result, and the errors that might enter into it. I am not in a position to form any judgment upon the mode of taking the thrust, but I am really unable to see any defect in it. The method of taking the thrust by means of those cylinders seems to me to be perfect. I think we owe very much to Mr. Yarrow for having made these experiments and communicated them, because, as Mr. Froude said, they lie quite outside any former experience. In the experiments with the Greyhound the proportion between the size of the ship and its speed was so totally dissimilar to what it is in this case that the production of such information throws altogether a novel light upon the question of resistance. I had rather hoped to hear from Mr. Froude some remarks as to the relation of these experiments to his own; I did not gather that he said much about them; but as his experiments went to much higher speeds than those of Mr. Yarrow in this case, one would like to know how they compare, and whether he found them fit in as Mr. Yarrow supposed they did. It is not clear to me whether Mr. Yarrow had the information about Mr. Froude's trial with sufficient accuracy.

Mr. YARROW. Oh, yes.

Sir EDWARD REED. I understand that point is clear. Then Mr. Froude has no further light to throw upon this important question at the higher rates of speed.

Mr. R. E. FROUDE. I should like to say one word in explanation. Mr. Yarrow's curves of resistance did not go above a certain speed; therefore we cannot make a comparison between that resistance and the resistances calculated from the model. I do not see how we can make a comparison at any higher speeds. We have the resistances of the model, but we have not the resistance of the ship to correspond.

Mr. W. H. WHITE. My lord, I have had an opportunity, through Mr. Yarrow's kindness, of seeing what was much more interesting even

than the diagram, namely, the whole of the apparatus itself, mounted and in working order, and I should like to say that, having looked through the apparatus carefully, I think, so far as the dynamometrical arrangements go, they were excellent, and the result of the experiments which were made to eliminate the internal friction, and so on, may be considered as trustworthy. I think Mr. Froude named all the principal points in which these experiments are of great value. It is the second large-scale experiment that has been made, and the only one where dynamometrical comparisons have been possible between the vessel and the model, and although we have every reason to be satisfied with model experiments as guiding us in the largest power required to drive a full-sized ship at a given speed, there is one element on which we want much more extended information, and in which only large-scale experiments can be trusted, viz, the ratio of the effective to the indicated horse-power. What Mr. Froude says is most important. I think its value will be appreciated by members when they come to read this paper, and refer back to the papers contributed by the late Mr. Froude to our Transactions, on the ratio of effective to indicated horse-power. Here we have a case, as is obvious from a glance at the diagram, where the ratio of effective to indicated horse-power is exceedingly high. And we have more: Mr. Yarrow has given us not merely the tow-rope resistance, as far as he could ascertain it, of the vessel, but he has also given us a second curve, which really includes the augmentation—a point to which Mr. Froude did not allude, but which was in his mind; that is to say, taking the curve "3" in the diagram, you have the force required to tow the vessel; taking the curve "2," you have the thrust delivered on the screw-shaft; the difference between the two shows clearly what the screw is doing in augmenting resistance. And in that respect, I think, if gentlemen when they have the opportunity will come and look at this model, they will see that Mr. Yarrow took a great deal of pains to make the difference between the curves "2" and "3" as little as possible, and that it has had a very excellent effect in some of these marvelous results he has attained in the steam trials of torpedo-boats. When we compare the ratio of the ordinates of these two curves, remembering that the late Mr. Froude estimated that the difference between the two was 40 per cent. in the Greyhound, those two things, set side by side, indicate what a remarkable gain there is, owing, as Mr. Yarrow well said, to the shape of the stern and the position of the screw. I did hope that we should have anticipated Mr. Yarrow—I am speaking now, if I may say so still, as an Admiralty officer—and have towed torpedo-boats up to the maximum speeds at which they could steam, and have obtained the information in that way. I hope that will be done, and then the information which Sir Edward Reed very properly asked for will be available. Of course it is quite evident that there may be a very considerable deviation in the ratio of effective to indicated horse-power between what you have when you get to the higher parts of the curve

and what is indicated within the speeds of these experiments. It was with the greatest difficulty that Mr. Yarrow could be induced to believe that the facts which he has put before us, and which, I think, are so full of value and instruction, would be sufficiently valuable to put into a paper. I can only say that I fancy I have done some little service to the institution in convincing Mr. Yarrow that his modesty was altogether misplaced.

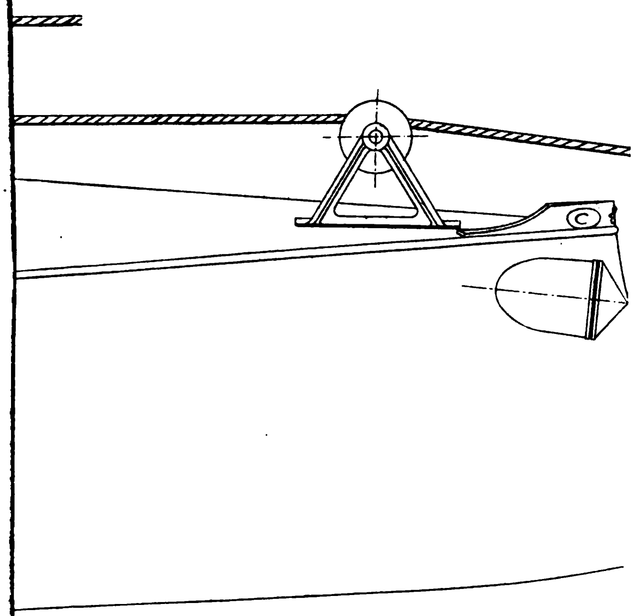
Mr. C. HALL. My lord, I should like to ask this question of Mr. Yarrow: Whether his experiment was made in a tideway or in absolutely still water, or whether there was any possibility of disturbance from the tideway? This may perhaps have been explained already, but having come in during the reading of the paper I have not heard it all. It is a fact well known to every one who has made trials on the Thames, or indeed on any stream, that if the current or tideway is ever so small it will always make a considerable difference in the result, inasmuch as you have the tide helping you for a short time, while it retards you in returning for a long time. Possibly this may be the explanation of the small difference of 3 per cent. between the results as found on the actual torpedo-boat and the results from the models at Torquay. As I said before, it may be that my inquiries are unnecessary; all this may be explained in the paper; but, as I have not read the paper, I cannot tell whether it is there or not. In a trial of this kind it would seem to me to be an important point, and it is worth while, therefore, to ask the question.

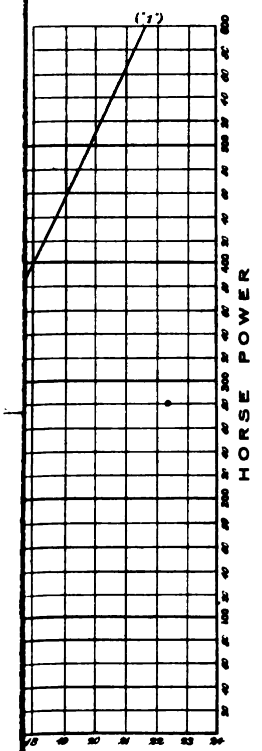
Mr. YARROW. My lord, Mr. Froude mentioned that the difference between the net resistance of the boat and the indicated horse-power did not appear to him to leave a sufficient margin. I fully agree with Mr. Froude; they do not apparently leave sufficient margin. All we have done in collecting these particulars of resistance is simply to describe the experiments, and Mr. Froude will notice that I have thrown doubts over the reliability of indicated diagrams taken at these high speeds. One reason why I do so is this: If we were to take a large diagram at a very high speed, say two or three inches high, we should find the diagram very unreliable. Therefore, I think we may assume that those sources of error, owing to the long stroke of the diagram, would more or less arise in the small diagram, although, of course, not to the same extent. Therefore, on these points I quite agree with Mr. Froude, and do not believe altogether in diagrams taken at these very high speeds; but at present it appears we have no other means of dealing with the subject. Another reason is, that if we compare engines with a short stroke, making a great number of revolutions, and apparently using about the same amount of steam, with a long-stroke engine, we do not find the diagrams agree. That rather confirms the opinion that diagrams at very excessive speeds are not to be relied upon, and I think, as a rule, those diagrams indicate less than they ought, which would fall in with what Mr. Froude says. As far as the

accuracy of these curves is concerned, whether they are accurate or not I cannot say; I have simply described the experiments; but I believe the line showing the thrust is more to be relied upon than any other, because everything went on very straightforwardly with these trials, and the whole affair was very simple, and in repeating the experiments two or three times we did not find the discrepancies that we did with the other trials. I will now allude to what Mr. Hall said about the boats being tried in still water. All these trials were conducted on calm days, but we had the tide to contend with. They were made at Long Reach. To endeavor to obliterate any errors that might arise with regard to the tide being favorable or unfavorable, what we did was this: We started with very low speeds and gradually worked up to very high speeds, and then we reversed the order of things; and on another day we started with a high speed—the same state of tide—and worked down to low speeds. This would tend to avoid errors which might be due to circumstances beyond our control, and which, I think, will meet the case Mr. Hall puts. Touching Sir Edward Reed's remarks about the position of the point of towing, I do not quite understand why, if we tow either at one point or at another, it should make any but a very trifling difference; consequently we towed from the point which was convenient to us, as shown on Fig. C, Plate I. Of course, whether that is a good point or a reliable point to tow from I do not know. I simply describe the experiments. Sir Edward Reed asked a question touching the trim of the boat. Now, the trim of the boat, when towed or when propelled, in all cases was the same when at rest; the trim was the same prior to being towed at 15 knots or propelled at 15 knots; but I cannot say whether there was any alteration in the inclination of the boat when moving at these speeds. We did not try anything to test that. I think, gentlemen, that is all I have to say, except to thank you for the kind way in which you have received my paper.

The PRESIDENT. Gentlemen, I am sure you will allow me to convey to Mr. Yarrow your united thanks for this very valuable paper. The discussion that has ensued upon it has been, of course, very interesting. I do not know whether many of you caught the expression that fell from my friend, Mr. White, which I will mention. He spoke as an Admiralty officer. You will, I know, all join cordially with me in the feeling of regret that it is probably the last time that he will address us in that capacity—I do not mean during this session, but in future sessions—in the capacity of an Admiralty officer. You will regret with me that the country is sustaining so heavy a loss in that department, and also congratulate cordially my great countryman upon the accession into his establishment of so much talent.

pedo Boat.





A

X.

A DESCRIPTION OF A METHOD OF INVESTIGATION OF SCREW-PROPELLER EFFICIENCY.

By R. E. Froude, *Associate.*

[Read at the twenty-fourth session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, president, in the chair.]

The main purpose of this paper is to describe and justify a particular method of investigation of the subject of screw-propeller efficiency by means of experiments on models.

The distinguishing characteristic of this method of investigation is that it divides the subject into two principal branches, viz: (1) the efficiency of screws working by themselves in undisturbed water; and (2) the manner in which that efficiency is affected by the screw being brought into conjunction with the hull of a ship, in virtue of the interaction of the screw and hull We may consider the operations which are the subjects of these two branches of the investigation as constituent elements of the complete phenomenon of propulsive efficiency, and for distinction we may term the former of the two the element of "screw efficiency proper," and the latter the element of "hull efficiency."

It is the latter of these two elements that has been the principal subject of investigation in the model screw experiments which have been in progress for some time past at Torquay—partly because it happens that this branch of investigation is the one which, under present circumstances, is most easily pursued with satisfactory results; partly because this element is, of the two, affected by the greater variety and intricacy of conditions, and is therefore the one most in need of experimental investigation; and partly because the relevancy of the results of an investigation of the "screw-efficiency" element must hinge upon the nature of the modifications introduced by the "hull-efficiency" element. The hull-efficiency element is, therefore, the principal subject of this paper.

Nevertheless, some investigation of the "screw-efficiency" element was, of course, a necessary preliminary to the study of the modifications arising from the actions of the hull, and to this extent the former subject will also have to be treated in this paper. The knowledge which we have as yet acquired on this subject may be said to be confined to (1) a tolerably accurate and complete determination (in model screws)

of those characteristics of performance which are common to all screws of ordinary pattern; (2) a general appreciation of the character of the differences in design of screw upon which efficiency depends, the amounts of the resulting differences in efficiency being as yet only vaguely determined.

At this point I think it desirable to answer a query which may perhaps suggest itself in reference to the foregoing explanation. Why, it may be asked, should the investigation be thus divided into two branches? Why, for instance, should not a model of any intended screw be tried solely behind a model of the ship for which it is intended, and the propulsive efficiency in the gross be thus ascertained by a single experiment? Such a question is not an unreasonable one, and I am quite prepared to admit that for obtaining such a result, if that were all that were desired, this direct method promises on general principles to be less laborous and more accurate than the more roundabout method of ascertaining first the efficiency of the screw in still water and afterwards the modifications due to its conjunction with the hull.

Of the reasons for nevertheless preferring the roundabout or analytical method, as it may be termed, some are incidental to mechanical difficulties in the experiments, and will appear as we proceed. Two important reasons, however, are based on general principles, and may advantageously be stated here.

In the first place, owing to the relatively excessive frictional resistances of both hull and screw-blades on a small scale, it is probable that the gross propulsive efficiency of model and screw, which is all that could be ascertained by the direct method above suggested, would differ by an unknown amount from that of the corresponding ship and screw. We could not even conclude with confidence that a comparison between the gross propulsive efficiencies of any two models would truly represent the comparison between those of the corresponding ships. The analytical method of investigation affords the only chance of determining the corrections (analogous to "skin-friction correction" in resistance) which are necessary in order to make the results of model screw experiments truly applicable to full-sized ships.

Secondly, and waiving the foregoing difficulty, unless in the use we propose to make of model experiments we are to be content with prophesying what will be the performance of a moderate number of individual intended ships with their intended screws, if in fact we aspire to be in a position to decide beforehand, in any case that may arise, what will be the best form of screw for a given ship, or what will be the best form of ship, taking propulsive efficiency into account, the number of combinations of different designs of hull and screw which it would be necessary to try in order to achieve this result on the direct or "gross" method would be altogether prohibitive. The analytical method is of advantage here, as it always is where the variety of governing conditions is great, by systematizing the conclusions and reducing them to

rule, and so enabling the result of any one of an almost infinite number of combinations of conditions to be inferred from the results of actual trial of only a moderate number of such combinations.

The advantages of an analytical system of experiment over one dealing only with gross results are in fact analogous to the advantages which a judicious system of book-keeping under headings possesses by comparison with a mere record of total receipts and expenditure, or, again, which an algebraical formula for the strength of a given class of structure has over a tabulated statement of strengths of actual structures of different designs and dimensions. In all these cases alike the statement of the facts in a comparatively complex and elaborate form is a necessary expedient for giving the required variety of information within a practicable compass.

The first step in the analytical investigation of propulsive efficiency is, as already stated, the division of the total efficiency into the component elements of "screw efficiency" and "hull efficiency." It appears convenient to preface the explanation of the constitution of these elements by some description of the experimental apparatus employed.

The screw on which experiment is to be made is mounted (see Fig. 1, Plate I) on the forward end of a shaft 3 feet 6 inches long, the bearings of which are bracketed down from a frame above the water level, and which is driven with miter gearing at its after end by a vertical spindle, the top bearing of which is in the above-water frame. This above-water frame is mounted on a delicate parallel motion, constraining it vertically and transversely, but leaving it perfectly free fore-and-aft-wise; and its tendency to fore-and-aft motion, which consists of the forward thrust of the screw minus the resistance of the mechanism in the water, is measured automatically by means of a spiral spring, recording its extension on a revolving paper cylinder. The whole is mounted on a truck running on the straight and level railway which extends throughout the length of the experimental tank a foot and a half above the water surface. The vertical spindle which drives the screw is driven by means of cord belts and a system of poly-grooved pulleys by the truck-wheels, so that by duly speeding these pulleys any desired proportion of rotary speed to linear speed of advance—or, in other words, any desired linear travel per revolution—can be rigorously assigned to the screw. Any desired linear speed can be assigned to the truck by the governor of the engine which drives it. The final cord belt which drives the spindle passes over a system of delicate levers and pulleys, by which the difference in the tension of the two parts of the belt (which is the measure of the turning moment applied to the mechanism) is automatically recorded on the same cylinder as the fore-and-aft force of the frame.

When twin screws are used, each screw has its own frame, the two frames being mounted, at their proper distance apart, on the same parallel motion, the driving belt passing successively over the sheaves on the two vertical spindles, so that the diagram in that case records

the sum of the net fore-and-aft forces delivered by, and the sum of the two turning moments applied to, the mechanisms of the two screws.

For eliminating the resistance of the frame-work, &c., to its passage through the water from the recorded fore-and-aft force, and the friction of the bearings, &c., of the mechanism from the recorded turning moment, so as to convert these measures into true thrust delivered by, and true turning moment applied to, the screw, various expedients are adopted, which space will not admit of my describing here. I should state, however, that while there is of course some room for slight inaccuracy in both these eliminations, the measure of true thrust is the most trustworthy of the two, and may be considered very accurate. That of the true turning moment appears liable to errors of some importance—sometimes of excess and sometimes of defect. The error, however, does not often vary suddenly or rapidly, so that comparative turning moment results of experiments included within a short interval of time may be trusted with more confidence than their absolute results.

In the experiments the truck carrying the apparatus above described is joined to the somewhat similar truck running on the same railway which is used for experimenting on the resistances of models, the model being for this purpose attached beneath it. When the two trucks are joined the model may either be attached in its place or omitted, and the screw experiments accordingly made either behind model or in undisturbed water, as desired; or, again, either the screw may be removed from the shaft or the trucks disconnected, so that the model also can be tried either alone or with screw working behind.

The apparatus that carries the screw measures the thrust, speed, turning moment, and revolutions per minute; consequently the experiments on screw working without model in front determine the element of "screw efficiency" proper; that is to say, they show the power expended in performing a given amount of useful work by maintaining a given thrust at given speed. The experiments on resistance of model alone measure the useful work to be done. The comparison of these records with those yielded by corresponding experiments with model and screw in combination show the modifications introduced into the results by bringing the model and screw into conjunction, and these modifications constitute the "hull efficiency."

Now, these modifications are two in number, viz, the effect of presence of screw upon resistance of model and the effect of presence of model upon efficiency of screw. The former effect consists in what has been termed the "augmentation" of the resistance of the model by the action of the screw, an effect which is necessarily a loss of efficiency, the amount of this loss being directly measured by the comparison of the resistances of the model with and without screw working behind.

The second mentioned effect consists in the fact that behind the model the screw is working in water in a certain state of motion, and

that, in consequence of this motion, to maintain a given thrust at given speed behind the model consumes a different amount of driving power (in all ordinary cases less) than in undisturbed water. This difference in consumption of power is, of course, indicated by the comparison of the recorded performances of the screw with and without model; but this comparison, when pursued in detail, indicates not the amount merely, but also the cause of the difference in consumption of power. This cause is the forward motion of the wake water in which the screw is working, and our experiments prove that, complex and varied as are actually the motions of this wake water, the net effect of this state of motion upon the screw is practically identical with that which would be produced by a mere uniform forward current, the forward speed of which, and the saving in driving power due to which, may be measured in a very simple and trustworthy manner by the method of experiment described.

To show how this is the case, we must study the matter in greater detail, commencing with the performance of screws in undisturbed water.

First, then, let us suppose a set of experiments with the apparatus already described, to be made with any given screw without model in front, at one linear speed but at various rotary speeds or revolutions per minute, ranging, let us suppose, from the revolutions which would about give no thrust (and at which, therefore, the screw would nearly turn of itself) to about twice that number of revolutions; in other words, from no slip to about 50 per cent. slip. The apparatus, it has been stated, measures revolutions per minute, thrust, and turning moment, and the results of the experiments may therefore be represented by diagram in the manner shown in Fig. 2 (Plate II), where the thrust and turning moment are plotted as ordinates to an abscissa scale of revolutions per minute.

We thus obtain curves of thrust and turning moment for a given screw at given speed and varying revolutions. Such curves possess the following characteristics, which are common to screws of almost every design. The thrust and turning-force curves both of course ascend with increasing revolutions. They are slightly concave upwards; but, nevertheless, do not become even approximately tangential to the base line, which, if they are produced far enough, they intersect at an angle: such intersection being necessarily at higher revolutions in the thrust curve than in the turning-moment curve.

Now for the measurement of efficiency. At any given number of revolutions per minute the energy in foot-pounds consumed per revolution in driving the screw is equal to the turning moment in foot-pounds of moment multiplied by 2π. The energy delivered per revolution by the thrust of the screw is the thrust in pounds multiplied by the linear travel or advance, per revolution, in feet. The efficiency, therefore, being ratio

of energy delivered to energy consumed, is the former divided by the latter, namely:

$$=\frac{\text{thrust} \times \text{travel per revolution}}{\text{turning moment} \times 2\,\pi},$$

or, which is a more convenient form—

$$=\frac{\text{thrust}}{\text{turning moment} \times \dfrac{2\,\pi}{\text{travel per revolution}}}$$

If, then, we multiply all the ordinates of the turning-moment curve by the factor $\frac{2\,\pi}{\text{travel per revolution}}$, we get a new curve, such that the ratio between its ordinate and the corresponding thrust-curve ordinate at any point in the diagram indicates the efficiency at that point. This curve is also shown in Fig. 2.

It will be seen that the effect of multiplying the turning moment by the factor $\frac{2\,\pi}{\text{travel per revolution}}$, is simply to convert it from absolute turning *moment*, or turning force measured at a unit radius of one foot, into turning force measured at a radius whose circumference is equal to the linear travel per revolution. The new curve, therefore, represents the turning force measured in this manner; for convenience we may term it the curve of "turning force," as opposed to "turning moment."

The "thrust," then, divided by the "turning force" (or turning moment $\times \frac{2\,\pi}{\text{travel per revolution}}$), measures the efficiency; the ratio of the ordinates of the thrust and turning-force curves may accordingly be measured at successive points in the diagram, and an efficiency-curve thereby constructed. Again, see Fig. 2.

The prominent characteristics of this efficiency curve are common to almost all screws. Its ordinate is necessarily zero at the point where the thrust is zero. and, like the thrust curve, it intersects the base line at an angle. It rises steeply and uniformly at first as the rotary speed increases, but rounds off and becomes gradually level at the point where maximum efficiency is obtained, whence it descends again gradually. The point of maximum efficiency is consequently somewhat indefinite, and it results that a given screw, advancing through water at given speed, is almost equally efficient within a large range of thrust value.

The diagram, or set of curves just described, viz, thrust and turning-force curves, and their derivative, the efficiency curve, exhibits completely the performance of a given screw at given linear speed of advance through water. Similar diagrams may of course be constructed for other linear speeds, and this might presumably be done by making several sets of experiments at various speeds, if necessary. There is, however, a very simple theoretical law for expressing the relation between such curves for different speeds, which experiment proves to be

very fairly correct, even for the greatest differences of speed that we ever have to deal with, and which, for minor differences, may be considered as absolutely accurate. This law we will now consider.

Suppose, then, that we commence with a diagram for a given speed $=V$, and that in this diagram, at revolutions per minute $=R$, we have thrust $=T$, and turning force $=F$. Let the speed be now changed to V_1. If the revolutions per minute remain $=R$ as before, the travel per revolution will be increased, the angles at which the various parts of the screw-blades cut the water will be changed, and the conditions of operation will be thereby metamorphosed in a manner which will defy accurate calculation. If, on the other hand, the revolutions per minute are changed in the same proportion as the speed, and become R_1 ($=R\frac{V_1}{V}$), the travel per revolution remains unchanged, so does the slip ratio, and so also do the angles at which all parts of the blades cut the water. The stream-line motions involved will consequently be unchanged in arrangement, the speeds at all points in the stream-line systems being simply changed in the same ratio as the linear speed of advance. The directions and relative proportions of all forces acting on all parts of the screw-blades, whether due to friction or pressure, will therefore remain unchanged, and their magnitude will be simply proportional to the square of the linear speed of advance. The items or component forces being alike changed in this ratio, the totals, or resultant forces of thrust and turning moment, will be changed in the same ratio, and so will also the "turning force," since this is turning moment $\times \frac{2\pi}{\text{travel per revolution}}$, and the travel per revolution remains the same.

In changing speed V into V_1, then, the original revolutions are multiplied by $\frac{V_1}{V}$, andthe original thrust and turning force are alike multiplied by $\left(\frac{V_1}{V}\right)^2$. The efficiency, therefore, which is thrust divided by turning force, remains unchanged.

The law of the relation between the performances of a given screw at two different speeds, is, in fact, in one way analogous to the "law of comparison," which expresses the relation between the resistances of ships and models, or similar ships of different sizes. Just as the latter assigns a ratio between the respective resistances, at certain respective "corresponding" speeds, and only at those speeds, so the former assigns a ratio between the respective thrusts and respective turning forces of a given screw at different speeds, at certain respective "corresponding" revolutions per minute, and only at those revolutions. These "corresponding" revolutions are proportional to the speed, and give travel per revolution constant and slip ratio constant; and at these "correspond-

ing" revolutions, the thrusts and turning moments are proportional to the square of the speed, and the efficiency is constant.

To illustrate as completely as possible the relations which these propositions establish between the diagrams severally expressing the performance of a given screw at different speeds, I show in Fig. 3 (Plate III) a series of thrust, turning force, and efficiency curves for a given screw at four different speeds. A_1 E_1, A_2 E_2, &c., are the several thrust curves, a_1 e_1, a_2 e_2, &c., the corresponding turning force curves, and aa_1 ee_1, aa_2 ee_2, &c., the corresponding efficiency curves. It will be seen that the four curves of each kind must be reproductions of one another on different scales, the zero of ordinate and abscissæ being common to all, the abscissæ scales of all being proportional to the speed, the ordinate scales of the thrust and turning force curves being proportional to the square of the speed, and that of the efficiency curves being constant.

It also follows that if any points B_1, C_1, D_1, E_1 be taken in the thrust curve A_1 E_1, and corresponding points b_1, c_1, d_1, e_1 in the corresponding turning force curve, at certain revolutions per minute; and if points at the "corresponding" revolutions to these—that is to say, at revolutions bearing the same ratio to the speed and giving the same slip ratio—be taken in the thrust and turning force curves of other speeds, as shown at B_2, C_2, D_2, E_2, and b_2, c_2, d_2, e_2, &c.; and if the successive points of equal slip ratio B_1, B_2, B_3, B_4, b_1, b_2, b_3, b_4, &c., be joined by curves, these curves will be parabolas, originating at the zero of revolutions. Also, if the corresponding points bb_1, cc_1, dd_1, ee_1, bb_2, cc_2, dd_2, ee_2, &c., be taken in the efficiency curves, the successive points bb_1, bb_2, bb_3, bb_4, &c., will fall in a straight and level line.

To still further complete the conception of the performance of a given screw at all speeds, which this diagram affords, we can imagine any additional number of thrust curves introduced so as to fill the whole diagram with a series of thrust curves corresponding to a series of minute gradations of speed. Any number of parabolas similar to B_1, B_2, B_3, B_4 may be likewise added, and the successive intersections of any one of these with the several thrust curves will be points of corresponding revolutions per minute, of identical travel per revolution, identical slip ratio, and identical efficiency. Each of these parabolas is, then, characterized by its appropriate efficiency value, which value may be determined by noting the ordinate of the efficiency curve for any one speed, at the point at which that parabola cuts the thrust curve for that speed; so that since turning force is thrust divided by efficiency, a single efficiency curve, in addition to the thrust curves and parabolas, is sufficient, without any turning force curves, to enable the diagram to show the whole performance of the screw at any speed.

I should here point out that the same train of reasoning also establishes a theoretical law for expressing the relation between the performances of similar screws of different absolute size; namely, that at

the "corresponding" revolutions, *i. e.*, the revolutions of identical slip-ratio, the efficiency is constant, the thrusts and turning forces being, for given linear speed, proportional to the squares of the dimensions. Here, however, we are immediately concerned only with the law for the same screw at varying speed.

Having thus prepared the way by a full consideration of the characteristics of the performance of a screw in undisturbed water, we are in a position to determine what its performance will be when working in the wake water astern of a model, supposing that wake to consist simply of a forward current having the same speed in all parts of it which come within the operation of the screw.

Supposing the speed of the model through the surrounding water to be V, and its speed through the wake water in which the screw works to be V_1 (so that the forward speed of the wake water is $V - V_1$), it is clear that the screw will be circumstanced precisely as when working in undisturbed water at speed V_1. If working at the same revolutions, the thrust will be the same, and so also will the turning *moment.* The linear travel or advance per revolution will, however, be greater in the case of the screw behind the model in proportion as V is greater than V_1, and the "turning *force*" (as measured for efficiency) will therefore be less in the same ratio. The thrust being the same, the efficiency of the screw working behind model will accordingly be greater than in still water in proportion as V is greater than V_1, and if we call the efficiency in the still water $=$E, the efficiency behind the model will be $E \times \frac{V}{V_1}$.

Or another way of stating the same result is to say that the screw being identically circumstanced relatively to the water it works in, the driving power consumed by it in the two cases is the same; but that the work realized is greater behind the model in proportion as V is greater than V_1.

The measure of the gain in efficiency due to wake is, then, the proportion between V and V_1, and at this speed V_1 in undisturbed water the thrust of the screw is the same for the same revolutions as behind model at speed V. Now, from what we have seen above (see Fig. 3) concerning the performance of screws in still water it is obvious that a given thrust with given revolutions in still water (a given single "spot," in fact, on the diagram shown in Fig. 3), is in a given screw, consistent with but one linear speed, and no other. Consequently, just as we may say that, given speed of model V and speed of wake $V - V_1$ the thrust for given revolutions behind model will be that appropriate to speed of screw V_1 in still water; so we may say, conversely, that given speed of model V, and given the fact that the thrust and revolutions of screw working behind model are those appropriate to speed of screw V_1 in still water, $V - V_1$ must be the speed of the wake.

If, therefore, we find that with model speed V we get a certain thrust for certain revolutions with screw working behind it, and that we get

the same thrust for the same revolutions in still water at speed V_1, we know that the speed of wake must be $V - V_1$, and that the efficiency of the screw when working behind the model must be its efficiency in the still-water experiment multiplied by $\frac{V}{V_1}$. The criterion of the speed of the wake, therefore, and of the consequent gain in efficiency, is the comparison of the thrust curves given by the experiments behind model and in still water. And it should be noticed that in order to determine this speed V_1, at which the required relation of thrust and revolutions subsists, it is not in practice necessary to actually try the screw in still water at exactly that speed (which might involve a lengthy process of trial and error), because the propositions already detailed concerning the relation of the performances of a given screw at different speeds, enable the thrust curve for any desired speed in still water to be inferred from experiments at a somewhat different speed.

These propositions concerning the effect of the wake of the model upon the performance of the screw would be strictly true if the actual wake were, as here assumed, a uniform homogeneous forward moving current. They will still be actually true in so far as the motions of the actual wake are in effect equivalent to this. Now, our experiments go to prove that complex as are the actual motions of the wake water, their net effect upon the operations of the screw does not in fact materially differ from that of a homogeneous forward current. Let us now consider to what this statement amounts.

What are the facts yielded by the experiments behind model and in still water taken together? We have (1) the thrust and revolutions of the screw working at speed V behind model; (2) the thrust and revolutions, each identical with the foregoing, at speed V_1 in still water; (3) the turning moment at the same speed in still water, which we generally state in the form of turning force as measured for efficiency. We then infer, on the assumption of a uniform wake, that the turning moment behind the model will be the same as in still water, and that the turning force will therefore be less in the ratio in which V_1 is less than V. But we also have recorded by the experiment (4) the actual turning moment behind model, which in the same manner we convert into turning force; and the degree of agreement between this actual turning moment or turning force (it does not matter in which form the comparison is made) with that calculated from the experiments in still water is a criterion of the sufficiency of the assumption that the disturbance of water which we term the "wake" amounts in effect to a mere uniform forward current. Now, this comparison has been made in a vast number of experiments, including a great variety of conditions differing in character and amount of wake, and the agreement, though not in all cases quite exact, is so nearly exact as almost to be covered by the limits of error of the observations. The general direction of this difference, such as it is, is the opposite of what one would be at first inclined to expect, since

it shows that the actual turbulent wake left by the model increases the efficiency of the screw more than would the hypothetical uniform wake, the efficiency given by the turning force recorded in the experiment behind the model being higher than that calculated from the experiments in still water by an amount varying from *nil* to about 2 per cent. in all the various conditions which have been subjected to experiment. I believe the variations, at any rate in the amount of this difference, to be due chiefly to inaccuracies in the record.

I am, therefore, certainly of opinion that although at some future day it may prove instructive to study the effect of the turbulence of wake upon the efficiency of screws in addition to the effect of its general forward motion, it is certainly desirable to leave the former element out of account until other vastly more important operations have been investigated.

It accordingly amounts to this, that in order, in the case of any individual model and screw, to determine the "hull efficiency," or, in other words, the propulsive efficiency in so far as it is affected by the characteristics of the hull and is not determinable by experiments in still water, the only records we have to make use of are (1) the resistances of model with and without screw which give the loss by "augmentation" of resistance, and (2) the thrusts of screw with and without model which give the gain by "wake." Records of turning force behind model are not required at all, and although in most of the experiments which I have made such records have been taken for the purpose of testing the validity of the hypothesis on which the measure of the "wake" gain is based, they serve no other purpose. The resistance and thrust records above mentioned evaluate the two elements of "augmentation" and "wake" which together constitute the "hull efficiency," and whatever further information is required to determine the total propulsive efficiency can be obtained from experiments with screws alone.

The experimental investigation of the whole subject of propulsive efficiency accordingly divides itself into two sections: (1) Experiments on screw thrust and model resistance to determine "hull efficiency," and (2) experiments on thrust and turning force of screws in still water to determine "screw efficiency." I will presently show how this system of inquiry is put in practice, and what are the conclusions of practical importance it may be made to yield; but it is first necessary to describe rather more fully the form in which the resistance and thrust records which determine the hull efficiency present themselves.

Fig. 4 (Plate IV) is a typical diagram showing the results of a single set of such experiments, viz, the complete set necessary for ascertaining the "augmentation" and "wake" values for a single model with its screw, at one speed which we will call V. The set comprises the three essential kinds of experiment, viz, (1) experiments on model without screw; (2) experiments on model and screw together; (3) experiments on screw alone.

The last two kinds of experiments, namely, those made with screw, each comprise several successive experiments at different numbers of revolutions per minute. The recorded forces in these are plotted as ordinates to a base of revolutions, and the "spots" so given are joined by curves. We thus get three curves: (1) the curve of thrust of screw behind model; (2) the curve of corresponding (augmented) resistance of model; (3) the curve of thrust of screw in undisturbed water. The ordinate representing the resistance of the model without screw may be indicated by a level line drawn across the diagram.

The experiment being made for the purpose of obtaining the "augmentation" and "wake" for a particular speed of model, it is essential that the experiments involving the model should be made at that speed. But there is no necessity that the experiments on the screw without the model should be made at any exact speed. For convenience it is desirable that these experiments should be made as nearly as possible at the speed V_1, viz, the speed of model minus the speed of wake; but all that is really necessary is that the speed should be sufficiently nearly equal to V_1 to enable the thrust curve for the speed V_1 to be inferred by means of the theoretical law of the relation of the thrust curves of a given screw at different speeds.

Now, when experiments such as we are here considering are being made, it is generally the case that the curve of resistance of model without screw at varying speed has already been ascertained; not unfrequently, also, the thrust curve of the screw in still water, at a speed sufficiently near to V_1, has already been ascertained for other purposes. It may be thought that in such cases the special experiments on model without screw and screw without model, to compare with those with model and screw together, might be dispensed with. But it must be remembered that in the experiments we are now considering, we are concerned with not the absolute values of the resistance of model and thrust of screw, so much as with the differences between those values when model and screw are separate and when in combination; therefore, in view of the slight gradual or occasional changes in resistance of model and in thrust curve of screw which are found to arise (mainly, as I imagine, from variations in surface-resisting quality), it is desirable to make all the experiments concerned in the measurement of the "augmentation" and "wake" in any given case, within a short time of each other. It is, therefore, generally necessary to take special experiments on model without screw, and screw without model, to compare with each set on model and screw in combination.

Let us now examine the character of the curves which exhibit the results of these experiments. We will first deal with the results of model and screw in combination, which consist of a thrust curve of screw, and curve of corresponding resistance of model. The point to be noticed here is that as the thrust increases, so also does the augmented resistance, thus showing that the augmentation is not due to the mere pres-

ence of the screw in its position, but to the amount of thrust produced, the suction exerted in front of it, which causes the augmentation, being the essential counterpart of the sternward ejectment of water, which produces the thrust. The augmentation is therefore a function of the thrust, and we ought to regard it as essentially a percentage drawback or discount from the generated thrust, whereby the thrust available for overcoming the natural hull resistance, is less than the thrust actually generated. It is therefore better to term it "thrust deduction" than "resistance augmentation," and the term "thrust deduction" has for this reason superseded "augmentation" in the notation used in the experimental establishment at Torquay. I shall therefore use the term "thrust deduction" in the remainder of this paper. The "thrust deduction," then, stated as a factor of the total efficiency, is the discounted thrust (or thrust *minus* augmentation of resistance), divided by the total thrust; or, stated as we more commonly state it, as a percentage, it is the percentage of the total thrust whereby the discounted thrust is less than the total, or in other words, the percentage whereby the value of the "thrust-deduction" factor is less than unity.

The absolute amount of the augmentation or "thrust deduction" is not, however, exactly proportional to the thrust; it may be more truly described as consisting of two terms, one proportional to the thrust, and the other a constant, this constant being the value of a certain small amount of "thrust deduction" which is found to exist even when the thrust is zero, as indicated by the dotted lines in Fig. 4. The explanation of this phenomenon lies apparently in the fact that the forward speed of the wake is not uniform throughout the sectional area operated on by the screw, whence it arises that when the slip of the screw in reference to the mean speed of the wake is zero, this mean slip is the resultant of a positive slip in some parts and a negative slip in others, so that in some parts the screw is creating thrust and making suction in front of it, and in others is conversely backing water and making pressure in front of it. If these regions of diminished and increased pressure in front of the screw were equally close to the surface of the hull, we should expect the consequent resisting and assisting forces on the hull to balance one another, and so produce no net effect. We know, however, that it is the part of the wake nearest the surface of the hull that must have the greatest forward speed, consequently the regions of positive slip, and diminished pressure in front of screw, must be nearer the surface of the hull than the regions of negative slip and increased pressure, so that the former must operate more effectively to increase resistance than the latter to diminish it.

We now come to the consideration of the two thrust curves of the screw with and without model. These will more or less closely coincide, according to how closely the actual speed of the experiments without model, and which we will call U, approximates to the speed V_1 or speed

of model *minus* speed of wake. If the two curves coincide, then $U = V_1$, and the wake factor of efficiency $= \frac{V}{U}$. If they do not so coincide we have to determine the value of V_1 by calculation from the known value of U, and this is done by the following construction, which is based on the propositions concerning the relation between the thrust curves of a given screw at different speeds.

In the thrust curve behind model (again see Fig. 4), take a point A, corresponding to revolutions per minute $= R$, say. Through this point describe a parabola, originating at the zero of revolutions (*i. e.*, such that the ordinates vary as the square of the revolutions), continuing it so as to cut the thrust curve of screw without model at the point B corresponding to revolutions $= R_1$, say; then $V_1 = U \frac{R}{R_1}$; the wake factor, consequently $= \frac{VR_1}{UR}$. This is the "wake" value for the revolutions corresponding to the point A; and in the same manner the wake value for revolutions corresponding to any number of other points C, E, G, &c., may be found by describing the parabolas CD, EF, GH, &c. Now the wake is a creation of the model, not of the screw; there is therefore no *prima facie* reason why the wake should be different for different revolutions of screw, and we should accordingly expect to find that the thrust curves with and without model coincide throughout if at all, and if not at all, that the revolutions corresponding to the points D, F, H, &c., bear severally the same proportions to the revolutions for points C, E, G, &c., as the revolutions for B do to the revolutions for A. As a fact, however, this is not always the case; the thrust curve behind model, in the largest class of conditions we have subjected to trial, being somewhat less steep than that which would give the above result, thus indicating that the wake diminishes slightly with increase of thrust. In another (but a smaller) class of conditions the difference is the other way, the wake increasing with increase of thrust.

The cause of this result, like that of the variation of thrust deduction with variation of thrust, already referred to, appears to lie in the non-homogeneity of the wake. This non-homogeneity results in the fact that the mean forward speed of the portion of the wake comprised within the disk area of the screw often differs from (is generally greater, though sometimes less than) the mean forward speed of the zones surrounding that disk. Now, when the screw is working with slip, and exerting thrust, there can be no question that the supply of water to it demands a convergence of streams towards it from without its own disk area, and that the degree of this convergence, and extent of the surrounding water affected by it, must increase with increasing slip or thrust of screw. Consequently, it is evident that the extent of the wake dealt with by the screw will be dependent on the thrust, and conse-

quently the value of its mean effective speed will increase or diminish as thrust increases, according as the speed of the surrounding zones is greater or less than that comprised within the disk area of the screw.

Thus, although, as was pointed out above, we may advantageously assume that the non-uniformity of the wake in no way invalidates the measure of the effect of the wake upon the efficiency, which the experiments afford on the assumption of a uniform wake; nevertheless, we see that this non-uniformity does appreciably affect the general result, by causing the values of the "thrust-deduction" and "wake factors," so measured, to vary with variation of thrust in an unexpected manner.

These values, as obtained at different points in the diagram, may be plotted to the revolution values to which they belong, as shown in the diagram, thus yielding curve of "wake" and "thrust deduction."

The diagram, Fig. 4, shows also two other curves of force record, viz, the curves of "turning-force" of screw with and without model. In describing this diagram I have hitherto avoided referring to these, because, as already explained, they play no part in the measurement of hull efficiency. I have shown them in the diagram, however, because thus far these records have almost always been obtained in our experiments for the purpose, as above stated, of testing the validity of the assumption that the effect of the actual wake upon the efficiency of the screw does not materially differ from that of the hypothetical uniform wake. An explanation of the way in which this test is applied will perhaps be useful, as an additional illustration of the meaning of this assumption.

Let us first suppose the speed U of the experiments without model to be exactly equal to V_1, so that the two thrust curves exactly coincide. Then if the wake were uniform its effect would be to increase the efficiency given by the recorded performance of the screw in still water by multiplying it by the wake factor as found from the thrust curves, or in other words, to diminish the turning force by dividing it by that factor. Hence, in so far as our assumption is correct, we should in such a case find the ratio of the ordinates of the turning-force curves, as actually recorded with and without model, to be exactly equal to the wake factor. In fact, the thrusts being equal, the turning force behind model will be less than that without model, in exactly the reciprocal of the ratio in which the efficiency behind model is increased by the wake.

If, however, as in this diagram, the two thrust curves do not coincide—*i. e.*, if speed U does not equal speed V—the first step is to infer from the thrust and turning-force curves without model at speed U, the turning forces corresponding, in still water, at speed V_1, to the thrusts recorded behind model, which is done thus. At the revolutions for the point A in the thrust curve behind model, we know the still-water efficiency for speed V_1 to be equal to that at the point B in the thrust curve without model at speed U, these being points of "corresponding" revolutions or identical slip ratio, and in the same way at the points C, E,

G, &c., the efficiency will be the same as at D, F, H, &c. Consequently the still water turning forces will severally bear the same ratio to the thrusts A, C, E, G, &c., as the ordinates *b*, *d*, *f*, *h*, of the turning-force curve without model do to the thrusts B, D, F, H, &c. We thus obtain the ordinates *a*, *c*, *e*, *g*, &c., forming a new curve *a c e g* of still-water turning force to correspond to the thrust curve behind model, and the ratio of the ordinates of this curve to those of the actual curve of turning force behind model should, as just now pointed out, be equal to the value of the wake factor as found from the thrust curves. In this diagram, which shows the actual result of one of our sets of experiments, the agreement happens to be exact.

As already stated, the agreement is not always so exact, the actual turning force behind being less than that inferred from the experiment without model, by an amount varying from *nil* to 2 per cent. of the total. I am certainly at present inclined to attribute at any rate the fluctuations in this difference to errors in elimination of the friction of the mechanism driving the screw. This suggests another point that should be noticed before we leave the consideration of these curves.

Assuming the object of an individual set of experiments such as we have been considering to be merely the determination of the total propulsive efficiency in the individual combination of model and screw with which the experiments were made (and we shall see presently that this is, at present at least, never the sole object, and rarely the most important object, of such a set of experiments), we see that the results provide two alternative methods of measuring this total efficiency, which two methods, in the case shown in Fig. 4, give the same result; as indeed, if the records are accurate, they are bound to do in so far as the assumption in reference to the wake effect is a correct one. One of these methods bases the estimate of total efficiency on the efficiency of screw behind model as actually recorded; the other on that efficiency as calculated by multiplying the recorded efficiency of screw without model, by the "wake" factor found from the thrust curves.

Now, it was stated that any individual set of experiments taken nearly at the same time are occasionally liable to a general error of tangible amount in the measure of turning force, owing to the difficulty of accurately eliminating the friction of the mechanism. To this kind of error either of the two above-mentioned methods of estimating the total efficiency in any individual case may be equally liable; and of the two, the first method, which bases the estimate on the efficiency actually recorded behind model, would appear to be preferable, as excluding whatever small error may be involved in the assumption respecting the effect of "wake."

But it is generally the case that the set of experiments with screw without model is only one of a large number of similar sets, with the same screw (and sometimes with others very similar to it) taken on different occasions to compare with experiments either behind other models or

behind the same model in different positions or at different speeds. And although these various still-water experiments have generally been made at various speeds, yet by aid of the propositions described they can be reduced to a common speed and compared, and thus an average curve of efficiency for the screw in still water can be obtained, which is much better authenticated (in view of the occasional errors in the measure of turning force) than a curve of efficiency given by any individual set of experiments. By using the still-water efficiency given by this average curve, and multiplying by the wake and thrust-deduction factors, a more trustworthy estimate of the total efficiency can be made than by merely multiplying the efficiency given by the actual experiment behind model by the thrust-deduction factor.

Hence, what I have above termed the second method, thus modified, affords a better measure even of the total efficiency in any individual case than the more direct method of using the actual recorded efficiency behind model as the basis of calculation. I therefore consider that we are fully justified in regarding the measures of turning force taken behind models in various cases as useful only as a test of the general correctness of the assumption respecting the effect of wake upon efficiency; and for this purpose they are fairly well adapted, since the two records, with and without model, the comparison between which affords the test in question, being taken almost at the same time, it is probable that whatever error may arise in the turning force measure from the inaccurate elimination of the mechanical frictions will be common to both.

We will now consider what are the results of practical importance that may be obtained from the kind of experiments that I have been describing.

We have just seen that the element of screw efficiency should be obtained, not from the experiments behind model, nor solely from those without model taken in connection with them, but from an average curve efficiency otherwise obtained. In considering the experiments in connection with models, we are, therefore, concerned solely with the "hull efficiency," viz, with the "wake" and "thrust-deduction" factors of efficiency.

We have seen that these latter generally differ somewhat at different points in the diagram, as in Fig. 4. Now, the diagram at the point where the curve of thrust behind model intersects the curve of augmented resistance shows what would be the state of affairs if the model were propelling itself. At the higher values of revolutions the condition corresponds to that of the model propelling itself against a head wind, or if towing another model; or at lower revolutions, to that of the model if under sail. Of these conditions, the first mentioned (*i. e.*, when the thrust = augmented resistance, which, for brevity, we will term the condition of T = AR) appears at first sight the only important one. But in considering to what state of affairs in the ship this condition in

the model corresponds, we have to remember, that before applying the law of comparison of resistances of ships and models to the calculation of a ship's resistance from that of her model, a correction has to be taken off the model's resistance, on account of the excessive effect of skin friction on a small scale. The actual condition of $T = AR$ in the model, therefore, corresponds to the condition in the ship with resistance enhanced by foul skin. For comparison with the ship in her natural state, to take the model condition, not of $T = AR$, but of thrust (as reduced by thrust deduction) equal to resistance as corrected for skin friction, would be a step in the right direction, but would not be quite right either, because the amount of the wake depends largely upon the amount of the skin friction, and so, though we should then have the correct thrust to compare with that of the ship, we should have the excessive wake due to the excessive skin friction of the model.

There is, therefore, no condition in which we can consider that the model truly represents the condition of the ship with clean surface propelling herself, and in order to put ourselves in a position to estimate the efficiency in a ship from experiments on her model, our only resource is to undertake a systematic series of experiments on the wake and thrust-deduction in models, aimed at discovering the laws of the effect of variation in skin friction resistance upon amount of wake, and how far, if at all, such variation in wake affects the thrust-deduction also.

A large portion of the experiments upon models and screws which have been recently made at the Torquay establishment have been devoted to this object, and a report to the Admiralty upon the experiments made thus far is in process of preparation.

For the relatively excessive skin friction of the screw blades on a small scale, a somewhat similar system of correction is also needed before we can treat the measures of the element of screw efficiency proper, yielded by the experiments on model screws, as directly and accurately relevant to the performance of full-sized screws. Assuming, however, that all these systems of correction have been sufficiently determined, or assuming that for the present we consider the measures of efficiency of given screws and models yielded by the model experiments, even without these corrections, a sufficiently valuable indication of the probable efficiencies of the corresponding full-sized screw and ships, let us now proceed to consider how the system of investigation advocated in this paper may be worked to the best advantage for obtaining results of practical importance.

The work, which has generally to be performed by model experiments, may be thus described: A design of vessel is required to fulfill certain novel conditions, and experiments are thereupon made with models of various alternative designs, in order to discover the best design for the purpose. The information yielded by such experiments has hitherto been practically limited to the determination of the relative re-

sistances of such designs; we now purpose, by aid of screw experiments, to extend the information to the relative propulsive efficiencies. But the total propulsive efficiencies of these designs will depend upon the design of screw with which they are severally to be fitted; and this is a point which is generally undecided at the time when the designs are first under consideration, namely, at the time when the model experiments are most required. Even if provisionally decided, the design of screw is generally, in many important respects, open to subsequent reconsideration. Plainly, then, the model experiments should determine the propulsive efficiencies of the proposed designs, not with any particular screw, but with whatever may prove to be the most suitable screw for each; and they should also be capable of determining what that most suitable design of screw is.

Our object must be to obtain this information from the smallest number of experiments with each model that will serve the purpose. By means of the method of investigation advocated in this paper, and with the aid of certain general preliminary investigations, I think the requisite information may, in most ordinary cases, be obtained sufficiently for practical purposes from a single set of experiments in connection with each model at one speed, such as the set the results of which are shown in Fig. 4.

The sufficiency of these experiments for the purpose presupposes, in the first place, a sufficiently exhaustive general investigation of the subject of screw efficiency proper to enable us to determine by calculation, without fresh experiments, what will be the efficiency of a screw of any given design, dimensions, proportions, &c., when maintaining a given thrust at given linear speed of progress through the water it works in; or, again, to enable us to determine the design, proportions, &c., of screw that will maintain given thrust at given linear speed, with maximum efficiency. Such an investigation, perhaps, sounds an arduous undertaking; it is not, however, so formidable as may at first appear, if, at any rate, at this preliminary stage the more minute refinements of shape of blade, &c., may be neglected. The ratio of pitch to diameter and the number and width of blades are the characteristics of design on which the screw efficiency proper mainly depends; and considering that we have not practically to deal with screws of more than four or less than two blades, and that the practical range of variation of pitch ratio and proportionate width of blades is not large, the total number of different models of screws which it will be necessary to try in order to exhaust the subject sufficiently for practical purposes is not very great. The ground has already been in a great measure cleared for such an investigation by the experiments which have been incidentally made in still water on various model screws.

The sufficiency of a single set of experiments at a single speed, for the purposes above described, also presupposes some general knowledge of the laws of hull efficiency in respect of (*a*) how it is affected by

variations of speed, (*b*) how it is affected by such variations in design or position of screw as are possible in a given design of hull. The experiments already referred to, of which a report to the Admiralty is in process of preparation, have been in great measure devoted to the investigation of these points. The general conclusions deducible from this investigation may for present purposes be stated thus:

(1) The variation of the elements of hull efficiency with variation in speed is generally slight, and so far regular in its character that in most cases experiments on a model at about the speed corresponding to the intended working speed of the ship will be sufficient for practical purposes.

(2) Variation of number of blades or pitch ratio of screw does not practically affect the values of the hull-efficiency elements.

(3) On the other hand, variation of diameter of screw, or of its position in reference to the hull, does affect these values, and in a manner which we have not thus far been able satisfactorily to reduce to rule.

The process of determination of the probable propulsive efficiency of any intended design of hull will accordingly take the following form:

Supposing diameter and position of screw to be fixed by circumstances, we have only to make one set of experiments with the model with screw (any screw that we may happen to have of the correct diameter) in the correct position; and we shall thus determine the wake and thrust deduction factors (and their product, the hull efficiency) for the proposed shape of hull and diameter and position of screw. We have then to select, by aid of our previously obtained knowledge on the subject of screw efficiency proper, the design of screw that will give with maximum efficiency the required thrust at a linear speed equal to the intended speed of vessel minus the speed of the wake. This will be the most suitable design of screw, and the product of its still-water efficiency under these conditions into the wake and thrust deduction factors will give the total efficiency.

Supposing that there is room for any large variation in diameter or position of screw, we should (at present, at least) be doubtless unable to dispense with other sets of experiments on model and screw, to include the alternative conditions of diameter and position of screw. The cases, however, in which the practical conditions of design admit of large variations in these particulars do not appear to be numerous.

DISCUSSION.

Mr. W. H. White. It would be a very poor compliment to Mr. Froude to pretend that we could possibly discuss such a paper as this after hearing it read, even with his personal explanations. I am thankful to say that I had some previous acquaintance with the principal points raised in the paper, and I followed the general drift of the paper as it

has been read, but I do not think I could have done that had it not been for Mr. Froude's previous explanations to me. I am sure of this, that we have in this paper the beginning of a very large work—that is, the beginning so far as practical results go. As Mr. Froude has shown to us, it is really the outcome of a very large work which has been going on, and which I believe I am right in saying was begun by the late Mr. Froude.

Mr. R. E. FROUDE. Certainly.

Mr. WHITE. Almost one of his last on leaving England.

Mr. FROUDE. I think long before that.

Mr. WHITE. I meant in this form, with the present improved apparatus. The investigation has been continud by Mr. R. E. Froude, and amplified in a direction which, I am sure, does honor to him; for these arrangements, which we see here in diagrams, when seen in fact display an amount of ingenuity and care, not merely in their design, but in their manipulation, which very few men could possibly give. It is a distinguished honor, I think, my lord, to this institution that in Mr. Edmund Froude we to-day have so worthy a successor to his father. As regards the value of these inquiries, I think Mr. Froude will agree with me that, before we can give the utmost practical effect to these model screw experiments, we want some large-scale experiments, just as in the model experiments with ships the late Mr. Froude was so much re-enforced by the large-scale experiments which he made in the Greyhound; and here, if we can only supplement the model experiments with screw experiments which can be trusted—varying their position, their forms and types, and revolutions—then, what has been often the cause of either great disappointment, or, as Mr. John told us last night, not extreme satisfaction, but which yet remains a mystery, may be cleared up. I may say already that Mr. Froude has helped us extremely at the Admiralty, in many points where there was a doubt, in resolving that doubt by means of model screw experiments; and whenever fine forms and high speeds have to be considered, then to be able to make changes in a model which could not possibly be made, except at great expense, in a ship, must be a source of great economy and of great progress in designing.

Mr. C. HALL. I should like to add my meed of praise with reference to this paper. I think that what Mr. White has just said is quite to the point, and I am convinced that this paper contains the elements of a most valuable research with regard to screw-propellers. Mr. Froude is well continuing the efforts of his late father in the direction of explaining that which the late Professor Rankine began to investigate in 1865, viz, the relation of V_1 to V. This question has puzzled every one, but I believe that at last we may entertain some hope of solving the problem practically. It would be impossible, having had the paper such a short time, to discuss it with anything like accuracy; I can only speak of it in terms of praise and admiration. I hope to read it through, and in

time to understand it—I confess I do not quite do so at present—and then I have every anticipation of deriving great profit and instruction from it.

Mr. J. H. Biles. My lord, this paper illustrates, I think, the very pressing necessity there is of getting papers of this kind printed at a very much earlier period in the year. Perhaps I am as much a defaulter as any one in this respect, in the two or three papers I have had the honor to read before this institution, but I would like to make a suggestion in a practical form: that the council should lay down a rule that no paper, from whomsoever it may be received, should be printed or circulated or read at the meetings of this institution unless it be in the hands of the council at least a clear fortnight before the commencement of the meetings, and that the papers should be circulated to members in order to give them at least some days to read over and to thoroughly understand the paper that was to be laid before them. There is no doubt this may be a little difficult to carry out, but the practical effect of the present regulation is this: if a member of this institution promises to give a paper, he knows that he will be quite safe if he gets his paper in two or three days before the meetings; consequently, when other engagements which may be more pressing present themselves to him, and when he has to decide between these two as to which he shall first give his attention, he says the paper for the institution need not be in until such and such a time, and, therefore, it must wait; but if the council of this institution were to make it a rule that a paper would not be received if it was after a fortnight before these meetings, then the pressing nature of that engagement would be equal to, if not greater than, some other great engagement; and I think that would have great value in the discussion of these meetings and the science of naval architecture. I speak in that way because I am one of the youngest members of this institution, and presumably have to look forward to a longer connection with this institution than most of you; therefore, I am anxious to get that rule made a really working rule as soon as possible. This paper, my lord, adds very considerably to the inducement for private ship-builders to take up this question of model experiments. Model experiments at present have been few, and though they have been of value, they have not been of sufficient value to induce ship-builders to take up the question. We decided a few years ago to lay down a tank. Circumstances interfered in the shape of a fire in the establishment, which disorganized the place for some time, and we have not been able to undertake the construction of that tank; and until hearing Mr. Froude's paper, in giving my opinion on the question, I have not been able to advise the firm to undertake that large task, for the simple reason that experiments with models alone do not cover sufficient of the ground to make them of the value that the money laid out would lead one to suppose the[illegible] this paper of Mr. Froude adds so very largely [illegible]

ciency that one can determine by model experiments in relation to steamship trials, that I think really now the question has become a question for more than one ship-builder; it has become a question for many ship-builders; and, if I may be allowed to make the suggestion, it appears to me that it is almost a question now (of course, assuming that Mr. Froude's deduction in this paper are correct) for a body like Lloyd's to take up. They have taken up many questions in connection with ship-building, and I suppose one may fairly say that they have taken them up successfully; and it appears to me this is one of the questions they might take up. Mr. White referred to experiments with large screws, but where could you have better experiments with large screws than can be had day by day with ships of the mercantile marine, if these experiments were properly organized by some body like Lloyd's. If Lloyd's were to undertake the determination of the resistance of models and the efficiency of screw-propellers, the experiments with full-sized propellers could go on day by day. There are sufficient trials made to enable one to make in a very short time a number of experiments, and to obtain the results with but very little extra cost. Therefore, I think that this is a subject that might be considered when the members of Lloyd's committee have sufficient time at their disposal. I may be wrong in proposing it, and perhaps am a little too enthusiastic on the subject, but it appears to me, with the appreciation I have—perhaps it is too much—in favor of this subject, it is one that cannot be attended to too soon, because it involves such very large interests, and is really too big a question for one ship-builder. With all respect to Messrs. Denny and some of the large ship-builders, their experience in connection with steamship trials is, after all, very limited. They only turn ships out at the rate of about one in the space of two months, whereas the mercantile marine of this country turns out at least a ship every day, and therefore the opportunities for investigations of this kind would be very much increased if the subject were taken up by a body like Lloyd's committee, and trials were properly organized. Then, this paper also shows how very dangerous it is to act upon the results deduced from experiments which are made upon limited assumptions. Now, two or three years ago the late Mr. Froude read a paper here. It was a well-defined paper. On certain assumptions certain results were deduced, but the outcome of it in some respects was this, that many people who were connected with carrying out practical work ignored, to a large extent, the assumptions, but adopted the results deduced from those assumptions. I think I am within the mark when I say that one of the tendencies of Mr. Froude's deductions was this, that propellers were too large and had too much surface. The consequence was that a great many people reduced the diameter of propellers and reduced the surface. There have been several lamentable results due to this, and ther[illegible] [illegible]uld be well that the profession in general, on [illegible]ing this [illegible] [illegible]mmencing to act on the results which may

be deduced (I am not quite clear whether there are any results pointed to in this), to keep clearly in mind that fact, that, in addition to the results deduced, one must be thoroughly familiar with the assumptions on which they are deduced. I have in my mind the case of a ship of 5,000 tons, with a power of 4,000 indicated horse-power. In order to develop the horse-power easily on a small weight of machinery, and guided, to some extent, in that resolve by Mr. Froude's observations, the propeller was made some 17 feet in diameter. I think the members will agree with me that 17 feet of diameter for a vessel of 4,000 horse-power and of 5,000 tons gross, appears to be small. The speed of the ship was about 14 knots. The consequence was that that vessel did not attain as high a speed as would have been obtained with a larger propeller. The result appeared to be a good one, and the owners were quite satisfied; but the next ship which followed was a ship of 1,300 tons more displacement. She was of the same length, but she had 3 feet more beam, and on trial had 3 feet more draught. Her propeller was exactly of the same pitch, and about 3 feet more diameter. That is in opposition almost directly to the results deducted by Mr. Froude; and that ship, in spite of her having, I think, 1,700 tons more displacement, attained exactly the same speed for the same power. The propeller of the second ship was not too small, and the mistake which was made in the first case was in assuming that a propeller could not be made too small. I wish it to be understood I do not wish to detract in the least from the results obtained by Mr. Froude, because I have the highest appeciation of them; but what I wish to point out is this, that one should not deduce practical and direct results from the results which he deduces on certain well-defined assumptions unless these actually apply to the particular ships in which the results are being tried. Then there is this further question in connection with that: Mr. Froude's experiments are always made on the assumption of an unlimited amount of power available; that is to say, that the amount of power which he has to drive his screws is unlimited; but there is always a point in an actual ship where the power is limited, and that is the safety-valves of the boilers. You cannot increase and decrease pitch and determine absolutely the efficiency of a propeller in a ship like you can in the case of a model screw, because, although you may increase pitch and get greater efficiency, the engines of the ship are so formed that they will not take in more steam than the parts will admit. The consequence is that, although you may get higher efficiency as far as the screw-propeller is concerned, you may not be able to develop the power which the boiler is able to develop, and, consequently, that is an element of great practical importance which does not enter into the theoretical value of the efficiency of the propeller, but which in any question of altering the shape of the propeller, or affecting the design of a propeller in a new ship, must be taken into consideration. I must thank Mr. Froude for the very able and clear way in which he has placed this paper before us, and I only regret that the

members of this institution had not a proper opportunity of thoroughly digesting it before the paper was read, in order that they might not only have criticised it, but have added in a much higher degree to the praise which should be bestowed upon Mr. Froude for his paper.

Sir EDWARD REED. My lord, when you put to the meeting the question whether an extension of time should be allowed Mr. Froude, I felt, and I believe the meeting also felt, from the vote they gave, that this was signally a case in which the author might be allowed greater latitude, because it could not be expected that the meeting would be able to enter into a minute discussion on such a paper. I rather think Mr. Biles, in his latter remarks, has been criticising some other paper, not the one we have heard read this evening, because this paper is remarkably free from any possible element calculated to mislead any one, and is entirely devoid of anything like even a suggestion of proportions, or anything whatever as to the means of determining upon the size of screws. This paper, so far as the institution and the professional world is concerned, although not so far as the results of previous labors are concerned, is the commencement, as Mr. White well put it, of a very large subject. It seems to me to be the first considerable inroad into that desolate region of ignorance which besets our profession. I do not know any question in connection with naval architecture in which we are so much at fault as with regard to the determination of the screw-propeller in relation to the ship; and I have myself, at the present time, a difficulty with two ships which have been conditionally purchased. I am perfectly certain that if I could make a few experiments, wisely resolved upon, I could determine some points which with the ships themselves it is almost impossible to determine, because every time you make an experiment you spend a couple of hundred pounds, and you cannot afford to make expensive and multiplied experiments when you have to pay for them in that manner. I wish to be allowed to say a word or two in praise of this paper, for this reason. It goes a little against the line which Mr. Biles took in the latter part of his remarks, but he will not mind that. I was afraid, to tell the truth, when I heard Mr. Froude open this paper, looking at it for the first time, that I was going to lose myself very soon, because of the probable fact that a number of conditions and assumptions would begin to enter in which would defeat my interest, and make me willing to wait for a very long time before being able to turn the results to a practical account, because that is the effect, generally speaking, in connection with experiments bearing on naval architecture. Not unfrequently, I may say, with regard to calculations and theories touching naval architecture, so many assumptions are introduced one after the other that by the time you get to the end of them you have lost confidence, and are not willing to go forward on those lines. I noticed particularly the manner in which Mr. Froude discussed the composition, if one may so call it, of the wake, and the thorough recognition which he gave to the practical differences between

the assumed wake of a constant stream and the actual wake of a turbulent stream. His recognition of those differences was so thorough, and his references to them so instructive and encouraging, as to the integrity, if one may so speak, of his investigation, that I feel peculiar pleasure with it; and I am satisfied that this paper, after it is on the records of the institution, will be a most valuable production; in fact, we have had no more valuable, in my opinion. With regard to what Mr. Biles said about the production of papers, I do not think we ought to attempt to draw such a large deduction as to say that no paper should be admitted unless it is presented within a fortnight before the meetings are held. Of course it would be an immense advantage—it would have been to-day with Mr. Watts' paper—if we had had it in our hands before the meeting; and such a paper as that we might fairly claim to have had before; but with regard to a paper of this kind, I apprehend there would never be any close discussion, because there would not be much to discuss in it; all that is to be hoped for would be a few suggestions here and there. What I want to point out is that if we cannot discuss the subject-matter of this paper, we get it in our hands and on the record, and what is the consequence? That probably we shall have a paper by Mr. Biles on the same subject next year, and we shall get a succession of papers on the development of this question, whereas perhaps otherwise the commentators upon this paper would have been satisfied with a passing criticism at this meeting; so that the circumstances under which the paper has been presented are not wholly without their compensation. At the same time, I think the council might profit by Mr. Biles' suggestion, so as to press strongly for papers generally coming in a little earlier and getting into our hands a little sooner. I do not know that I can say any more, except to confirm what Mr. White says. It is to my mind a matter of the greatest possible satisfaction, and I am quite sure it is to this institution, to know that Mr. Froude is doing such thoroughly valuable work. In this he is following upon the labors of his lamented father; he is feeding the future profession very largely; and I earnestly hope that nothing will be said or done on occasions like this to discourage him in the great and valuable work which he has in hand.

Mr. BILES. You will allow me, my lord, one word in explanation. Sir Edward Reed says that I was criticising a former paper.

Sir EDWARD REED. I said you were not criticising this one.

Mr. BILES. I believe the presumption from that is that I was criticising another one. I wish to make it quite certain that I have nothing but commendation for the earlier paper of Mr. Froude, but what I do want to impress very forcibly upon the members is, that any deductions which may be drawn from this shall be drawn with a full knowledge of the assumptions upon which they are made. Sir Edward Reed has corrected me, and said that the assumptions are practically nothing.

Sir EDWARD REED. I have not said that.

Mr. BILES. That only gives force to what I say, that the papers should be earlier in the hands of members, in order that the time of the meeting may not be taken up by any one making a statement such as I have made, which would not have been made if I had seen the paper earlier.

Mr. W. JOHN. I would like to say a word or two, and I can indorse every word that fell from Mr. White and Sir Edward Reed as to the extreme value of this paper to a naval architect. Sir Edward Reed put the matter very cleverly when he said it opens out a hope of our getting from a state of ignorance into something like a decent state of light on the subject of screw-propellers. We know a good deal of the theory of naval architecture in many directions, but on this one question of screw-propellers I may say I don't know a naval architect who is not in as near a state of absolute darkness as it is possible to be in. Now, of course we can work tentatively from one ship to the other where the steps are not great, but as for knowing the absolute theory of the thing, and as for being able to argue in anything like a bold way with large steps, I think the whole of us are almost adrift, and hopelessly adrift, and I do welcome the fact of Mr. Froude taking up this question in the bold manner he is doing, in the spirit in which his father attacked some other questions in naval architecture that appeared insoluble and mastered them. I do feel more pleased that this is the particular question Mr. Froude has taken up at the present time than if he had taken up any other question in connection with the subject. Mr. Biles suggested probably Lloyd's would take up the matter as a means of feeding Mr. Froude with the records of the mercantile marine, and although I do not think it is probable Lloyd's committee would see their way to do so, I do think there is another way of doing it, and that is that the private ship-builders in the country should put their data and the result of their experiments and their trials freely in the hands of a man like Mr. Froude, and let him deduce what he can from them and check them by his own model experiments, and in that way we can get together in a comparatively short space of time an amount of data that will enable Mr. Froude really to settle this question, or at any rate to put it on a sound basis. Of course there comes the question whether private ship-builders will place their data in the hands of any one outside their own firms. Some builders are prepared to do it, and some do it freely at the present time; those who do not see the advantage of doing it in a case of this kind will simply not be able to appreciate the results when Mr. Froude has solved the question. And, I say, while fully recognizing the desirability of competition between private ship-builders, I believe that it would be better for all of us to compete in light than to compete, as we are at present, in darkness, and therefore it would be to the interest of all ship-builders in the country to give Mr. Froude, as freely as the Admiralty give him now, all our data with

only one condition, that his results shall come out publicly at institutions like these, as I know they will sooner or later, and that it should be a fair, open attempt all round to get at a solution of the most difficult and most complicated thing at present in connection with naval architecture.

The PRESIDENT. I must take the opinion of the meeting now as to whether you would wish me to close this discussion. I must point out to you that the original reading of the paper occupied fifty minutes or more, and it was thought that that latitude should be given, because the discussion itself would be a very short one. We have three papers more to be read, and therefore I must ask you to express in some mode whether you wish the discussion to go on or not.

It having been put to the meeting as to whether the discussion should be closed—

The PRESIDENT. I see that the feeling of the meeting is that it should be closed, and therefore I most unwillingly close the discussion.

Mr. R. E. FROUDE. I have very few remarks to make in reply, except to thank all the gentlemen who have joined in the discussion most cordially for the undeservedly complimentary strain in which they have spoken, and I can only say that I am very glad that the paper has given rise to so valuable a discussion, if not on the details of the subject dealt with, on the very important general question of the use of scientific investigation, that is to say, the manner in which it is to be used for practical purposes in naval architecture; and those remarks have suggested to me one word from myself in reference to the scope of this paper. It certainly professes to suggest a manner in which the work which is now done by experiments on models in reference to the question of resistance may be extended to the question of propulsive efficiency. It professes to suggest a way which I think is the only way of doing it, if it can be done, though I am not as yet certain that it can be done very effectively or successfully. The corrections which may be required to make model screw experiments fully applicable to full-sized ships may really baffle us; but I feel that, even if we are baffled in that attempt, this paper has a value, in that the propositions which I have put forward are a contribution to the science of the subject, and I think, in virtue of the light thrown on the question by the propositions, we shall be able to do a good deal in analyzing and systematizing the results which are to be obtained from full-sized trials with ships; and that we can thus make much better use of results obtained from steamship trials than can be made without the light which is thrown on the general question of propulsive efficiency by this investigation. (Hear, hear!)

The PRESIDENT. Those sounds are quite sufficient, and I am sure I may convey to Mr. Froude your most grateful thanks for his very valuable paper.

Plate I.

w-Propeller Efficiency.

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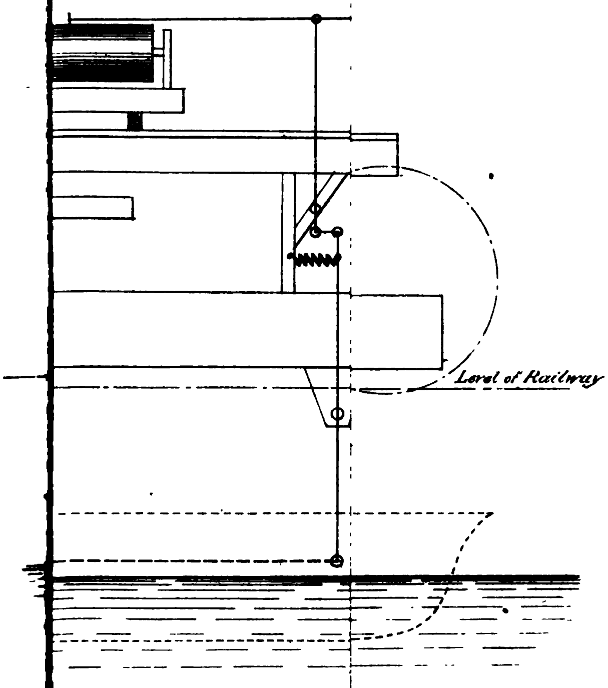


ller Efficiency.

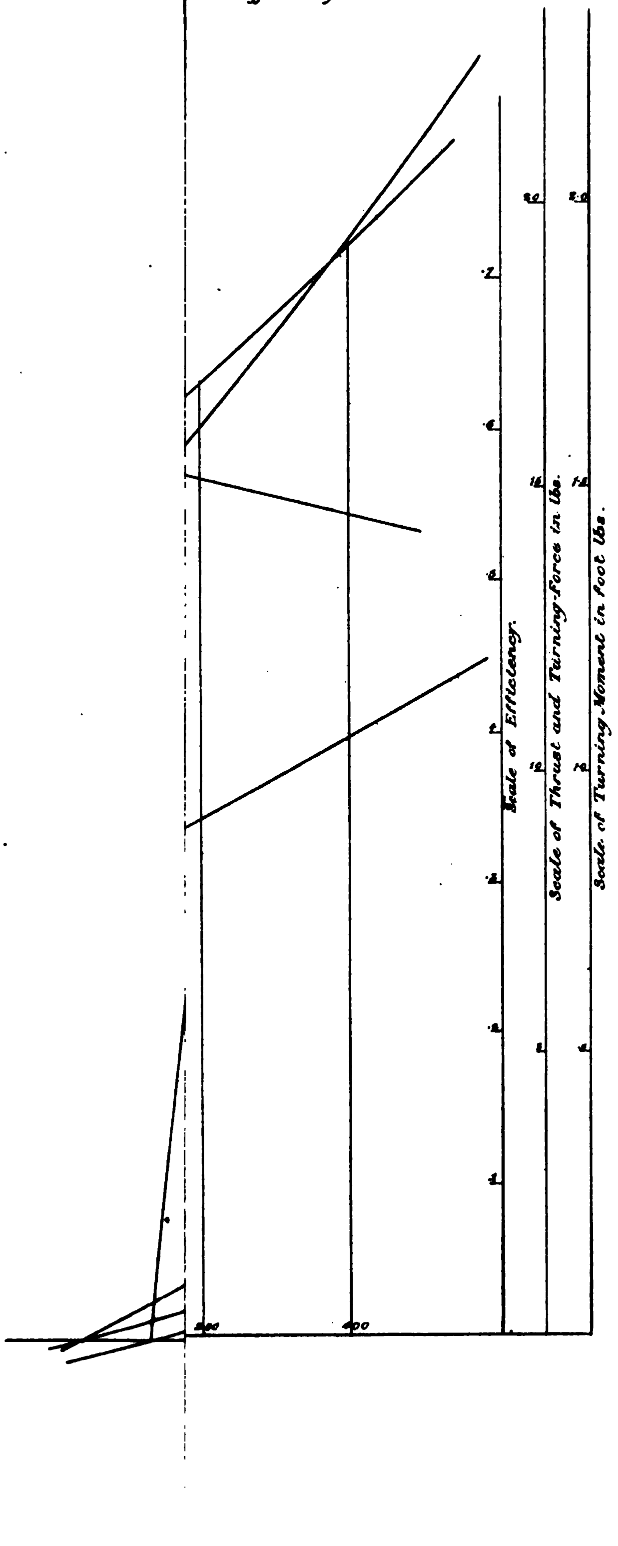
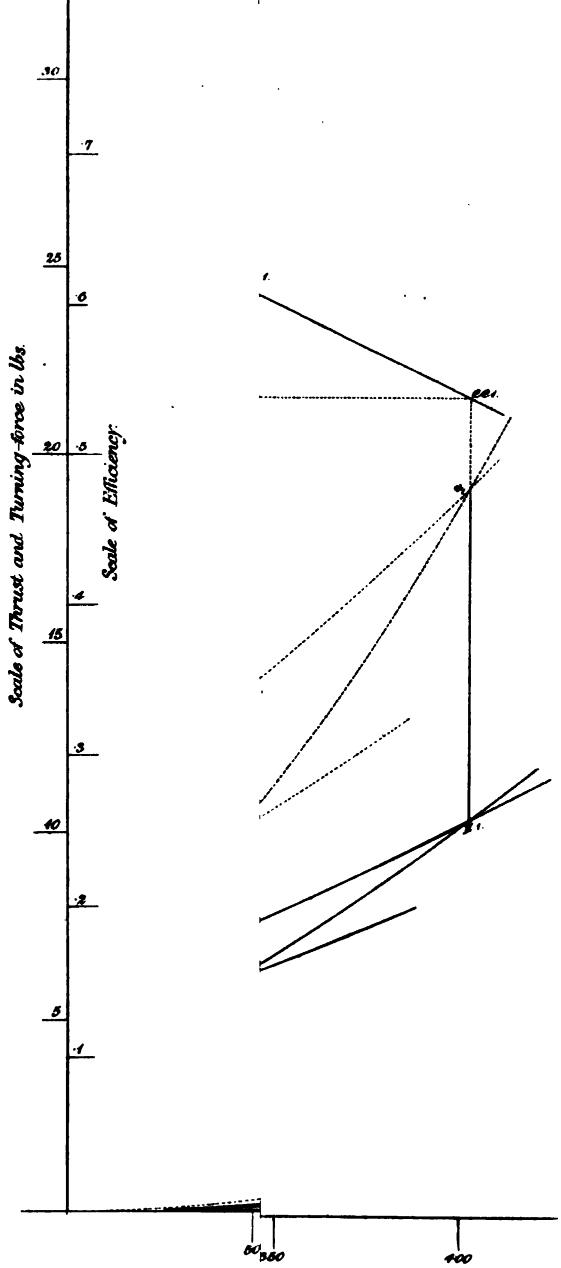


Plate III.

crew-Propeller Efficiency.



XI.

ON THE STEAM TRIALS OF THE "SATELLITE" AND "CONQUEROR" UNDER FORCED DRAUGHT.

By R. J. BUTLER, Esq., *Member*.

[Read at the twenty-fourth session of the Institution of Naval Architects, March 15, 1883; the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

The many advantages which are obtained in torpedo-boats and other small craft fitted with a single boiler, by the successful employment of forced draught by fans in closed boiler-rooms, have naturally induced the designers of war ships and their machinery to endeavor to secure similar advantages, by the application of the same system on a larger scale to the common high-pressure boilers, which are usually fitted in such ships.

For some considerable time past provision has been made in the designs of the machinery of all the most important ships of the Navy for the application of this system, so that it only remains to order the addition of the requisite boiler-room fittings, in case of its adoption.

The French Government have used the fan draught with ordinary boilers in closed boiler-rooms in ships of war for some two or three years, and in this country the firm of Sir W. Armstrong & Co. have adopted and carried it out successfully, in connection with low circular boilers, in some fast cruisers which they have built during the last two years for foreign Governments.

Within the last twelve months, the Satellite and Conqueror, fitted with boilers of the usual types, were far enough advanced to admit of the necessary additions being made to their boiler-rooms, for putting them under air-pressure, without delaying their completion and trials. The opportunities thus afforded of acquiring needed information were embraced; the arrangements have been carried out in these vessels, and several steam trials made.

The engines of the Satellite and Conqueror were not designed to develop the high powers which it was found the boilers could generate steam for, even when they were worked under a moderate amount of air-pressure. The conditions, therefore, were not favorable for the determination of the best possible results, but those which were obtained are of sufficient value to enable the capabilities of the machinery to be more nearly proportioned to the boiler power in other vessels. It is in

the hope that the particulars of the trials made in these two vessels may be of interest to the members of this institution that this paper has been prepared by me.

The Satellite is a single-screw composite sloop of 1,420 tons displacement. Her engines are of the usual two-cylinder compound type, placed horizontal. The boilers are of the long low kind, with two furnaces in each, an intermediate fire-box, and the tubes beyond. The boilers are arranged in pairs in two separate water-tight compartments, as shown on Figs. 1, 2, 3 (Plate I), being fired at the forward and after ends respectively. One funnel serves both sets of boilers. The engines and boilers are placed beneath a steel deck, which is below the water line. All the necessary openings in this deck for ventilation and other purposes are protected by shutters or stout bars, and coffer-dams. Direct communication between the boiler-rooms, and to the back ends of the boilers, is obtained by means of a passage at one side. An air-lock is provided at each end of this passage, and also at the entrance to the after boiler-room from the engine-room. Air-tight screens of thin sheet iron are worked flush with the fronts of the boilers, attached to the fore and aft coal-box bulkheads at the sides, and to the steel deck and ship's bottom. Doors are provided in these screens to give access to the tops of the boilers. Each boiler-room has a fan five feet in diameter, placed horizontal under the steel deck, with separate air-supply shafts extending well above the upper deck. Other air shafts and openings are provided for ventilation and access to the boiler-rooms, but these are closed when the forced draught is applied.

Two sister vessels, the Heroine and Hyacinth, have machinery and boilers exactly similar to those of the Satellite, except that they are not fitted with the apparatus for working under air-pressure. The particulars of their steam trials are given at the end of the paper, because they form an interesting comparison with those of the Satellite.

The Conqueror is an iron-clad ram of 6,200 tons displacement, having twin-screw engines of 4,500 estimated i. h. p. Her engines are vertical inverted three-cylinder compound, with cranks at equal angles. A middle line bulkhead separates the two engine-rooms. There are eight high boilers with return tubes over the furnaces. They are arranged in pairs in four boiler-rooms, separated from each other and from the engine-rooms by water-tight bulkheads. The boilers are placed with their backs to the middle line bulkhead, and are fired from the wings. One funnel is common to all the boilers.

The only additions made to the original arrangements in the boiler-rooms consist of a horizontal ceiling (Figs. 4, 5, Plate I), about eleven feet above the floor, worked across each room from the coal-box bulkheads to the fronts of the boilers inclosing the uptakes; and vertical screen plates between the boilers extend up from the front boiler bearers to meet the ceiling. The vertical plates are also worked round the fronts of the boilers inclosing the smoke-boxes, which thus keep

the boiler-rooms cool. These vertical plates are far enough back from the fronts of the boilers to form pockets for the gauge-glasses.

Hinged doors are fitted along the ceiling to be open under the normal conditions of working, so that the original system of ventilation by means of cowl pipes from above the upper deck then operates. Air-locks are provided between the engine-rooms and after boiler-rooms, and also the passage-way across the ship at the forward end of the other boiler-rooms. When working with all the boilers under air-pressure communication is open between the forward and after boiler-rooms through door-ways in the cross bulkheads.

Two vertical 4-foot fans on one spindle, fixed at the after ends of the rooms above the ceiling, supply each of the after sets of boilers; and one 5-foot fan at the forward ends of the rooms supplies each of the forward sets of boilers.

The air supply for the after fans is obtained entirely from above the upper deck through separate vertical shafts. The forward fans are supplied partly from the main deck and partly from the upper deck. The principal particulars of the machinery of the Satellite and Conqueror, as well as the most important of the observations made on their trials, are also given at the end of the paper.

The Satellite and her two sisters were all tried under natural draught alone. Trials were also made in the Heroine and Hyacinth, using steam jets in the funnels, to compare the effect with that of the fan draught. In the former vessel only the forward boilers were worked in this way, but in the Hyacinth ail the boilers were used, and they were worked as hard as possible in both cases.

The i. h. p. developed on these two series of trials agree with each other very fairly, considering that they were made under somewhat different circumstances. The intended horse-power of these vessels was 950, to be obtained without any forcing, but the result considerably exceeded this in each case, the mean power of the three, under natural draught simply, being about 20 per cent. greater than the contract power. This was owing, no doubt, to the exceptionally good ventilation, the high funnel, and to the high steam-pressure used. The performance was very satisfactory, but it had the effect of narrowing the scope of the working under forced draught in the Satellite.

These trials show that under the most favorable conditions, from 10 to 10½ horse power is obtainable per square foot of grate from this class of boiler, when worked without forcing the draught; and that nearly 13 horse-power, or about 24 per cent. more, can be realized when the ordinary steam blast is employed, the boiler-rooms being open as usual. It is to be observed that the power obtained by forcing the draught in the boilers of these vessels by steam jets, compared with the results from other similarly proportioned boilers, is also exceptionally high, which is only attributable to the favorable conditions above referred to. The proportionate increase of power produced by these means, over that given by the natural draught alone, is, however, not exceptional.

On the Conqueror's first trial, scarcely 8 horse-power per square foot of grate was obtained. But here the conditions were not so favorable for the development of a high result as in the smaller ships. The grate bars were unusually long, and the closed-in boiler-rooms rendered a very slight use of the steam blast necessary at times to quicken the draught. As the effect obtained from these boilers on this occasion was less than those which have been previously obtained under natural draught alone, from the same type of boilers similarly arranged, this can only be regarded as equivalent to a natural-draught trial. The load on the boilers was 20 pounds less than on those of the Satellite, but the boiler tubes are brass (ferruled at the fire-box end), whereas those of the Satellite are iron.

The performance of these boilers is, however, not really so bad as it appears to be, judging it with reference to the grate area. In comparing the effect obtained from exactly similar boilers, the grate area forms a safe basis of comparison; but in dealing with different types of boilers, such as those of the Satellite and Conqueror, this does not necessarily hold, seeing that in one case, as in that of the high boilers of the latter, the proportion of grate area may be abnormally increased, by extending the grate bars to the backs of the fire-boxes. The maximum grate area obtainable was employed in both types on these trials, that of the Satellite bearing nearly the usual ratio to the total heating surface, which is fixed, while that of the Conqueror was greatly in excess. The proportion of heating surface provided in both types of boiler to the estimated horse-power is practically the same.

A comparison of the power obtained per square foot of grate on the Conqueror's first trial with the mean result of the natural draught trials in the other vessels makes the performance of the boilers of the latter appear about 30 per cent. superior to that of the high boilers; but, comparing the mean power realized, in proportion to the total heating surface, as it properly should be compared, the apparent superiority of the low boilers becomes reduced to 9.5 per cent., and, as above mentioned, the favorable circumstances which obtained in the small vessels, and the unfavorable ones in the Conqueror, account for this.

The first of the Satellite's forced-draught trials, like the first steam-blast trial, was made with the two forward boilers only, to keep well within the limits of the capability of the engines to transmit the power. The air pressure used varied from 1 to 1½ inches of water, but was in effect only an inch throughout, the increase being required gradually as the heating surface became diminished by the lower tubes being blocked up by coal thrown over the bridges. 864 i. h. p. was realized as the mean of the three hours' work, or 15.7 horses per square foot of grate, and this power was maintained during the whole time as uniformly as could be expected. The tendency of the boilers to prime prevented this air pressure being exceeded.

On the next trial all the boilers were used, and worked for half an

hour at ½ inch air pressure. The results of this are only given as showing that this amount of air pressure with the boiler-rooms closed produced practically the same effect, in regard to power, as the natural draught with the boiler-rooms open.

On the subsequent trial three boilers were used, the air pressure being increased gradually. With ½ inch, the effect is very nearly the same as when using the steam blast.

At an air pressure of 1 inch, 16 horse-power was obtained per foot of bar, but it will be noticed that the condensing apparatus, which maintained a good vacuum up to about 1,200 horse-power, was being overtaxed, and a fall in the vacuum took place.

The three boilers were further worked at an air pressure of from 1¼ to 2 inches. With the increase in air pressure, the vacuum was further diminished, but the power and speed of the engines were maintained with a very fair degree of uniformity. Apparent unsteadiness in the water caused the feed to fluctuate, and necessitated variation in the air pressure to keep the power uniform. The i. h. p. now obtained reached as high as 1,397, or 16.9 per foot of grate bar, as the mean of the two hours' trial. This exceeds the performance under natural draught by 62½ per cent., and that under steam blast by 30 per cent. Considerable leakage of air occurred through the furnaces of the unused boiler, especially at the higher air pressures, so that an increased fan speed was necessary in one boiler-room to maintain the required air pressure.

After this, an attempt was made to work all the boilers, commencing with 1½ inches of air pressure, but the engines could not take the steam, and it was abandoned. The indicator diagrams taken showed that 1,570 horse-power had been developed, or about 65 per cent. above the specified power of 950 horses, the engines running at 126 revolutions.

The indicator diagrams were taken, and the usual observations made every quarter of an hour, during the forced-draught trials in this ship.

In the Conqueror two series of forced-draught trials took place. The first, which immediately followed the ordinary trial, was made solely with the object of ascertaining the maximum power and speed the vessel could attain on an emergency, and it is only valuable on that account. Steam blew off freely from the safety-valves during the whole time, and additional pipes were fitted to admit steam direct from the main pipes to the low-pressure cylinders. Thus a large quantity of the steam generated was wasted, and another large portion used uneconomically.

The other trials were made to determine the capabilities of the boilers, and for this purpose the after boilers only were used. They were worked for 1½ hours, under an air pressure varying from 1 to 1½ inches in the two rooms, and subsequently for the the same period at from 1½ to 2 inches of air pressure. The i.h.p.'s developed under these conditions were 3,665 and 4,023, or at the rate of about 12.2 and 13.4 horses

per square foot of grate respectively. The same length of grate bar, viz, 7 feet 6 inches, was used on all these occasions, and there is no doubt that the high boilers were worked at a considerable disadvantage on this account, as compared with the low boilers of the Satellite. But as one object of the trials was to ascertain the workability or otherwise of the long bars, they were retained. Unfortunately, circumstances did not permit of further trials being made with the bars shortened, so as to reduce the grate area to the usual proportion, for they would have yielded useful information.

If the performance of these boilers on the last trial, which was the best that, I believe, could be maintained under the circumstances, be compared, on the basis of the proportion of power obtained to total heating surface, with that of the Satellite's boilers when the greatest effect was realized, it will be seen that the latter still appear superior in steaming power, and to about the same extent as before, viz, 9.6 per cent., although the mean air pressure was slightly less. Comparing, again, the first trials of the Satellite and Conqueror under forced draught, and regarding the air pressures as practically the same, the performance of the boilers of the former appear 10 per cent. better than that of the high boilers. But on a short trial made in the basin at Chatham, using one of the after sets of boilers only and one set of engines, the grates then being 6 feet 6 inches long and the air pressure 2 inches, 2,140 horse-power was developed, which is at the rate of 16.46 per square foot of grate, and 0.625 per square foot of heating surface. This approaches more nearly the best of the performances of the Satellite, which is only superior to it to the extent of 2.56 per cent., but which was obtained with considerable less mean air pressure. It must be observed that on this occasion the fires were clean, and the men fresh, but as a considerable margin of power remained in the fans, it is an open question whether this power could be maintained as long as the other trials lasted.

It is certainly a matter of great importance in war ships to make the grates as long as the furnace barrels will admit, provided they can be efficiently worked, for long grates mean less width of boilers and reduced weights; but it is doubtful whether, on the trials of the Conqueror, the bars were not too long for the width of the furnaces; it was very difficult to fire the backs of the long grates effectively, owing to the thick fires which had to be maintained, and I do not think it was done. The fluctuations which occurred in the air pressure pointed to the conclusion that uniform fires were not being kept, for the feed was very regular—much more so, in fact, than in the Satellite, where the water was always more or less unsteady.

The total weight of the Conqueror's boilers, with all their fittings, is less than that of the Satellite's in proportion to the horse-power developed under natural and also under fan draught, and both sets of boilers are average specimens of their classes. (Double ended boilers would be

lighter still.) If their weights relatively to the power obtainable by using the steam blast could be compared, then probably the Conqueror's boilers would, as usual, appear heavier than the Satellite's, for the steam blast is not nearly so effective in producing increased power in the return tube boilers as in the low ones. The risk incurred in working high boilers under air pressure is undoubtedly less than in working low ones, but they require harder forcing, apparently, to produce the same effect; the difference in their steaming power, however, could easily be made up by using larger fans to obtain a little additional air pressure; further, seeing that the high boilers have also a slight advantage as regards weight, it would appear preferable to employ them in connection with the fan draught, rather than the low type, in vessels of war where the height and protection will admit.

The fan speeds necessary to maintain the different air-pressures were noted with the 4-feet fans of the Conqueror, and with the 5-feet fans of the Satellite, whilst the boilers were worked. Those of the Satellite, at 400 revolutions per minute, sustained 1½ inches of air pressure, and each addition of 50 revolutions produced approximately an increase of 0.3 inch. In the Conqueror, 600 revolutions gave an inch with the long grates, and 1.2 inches with the 6 feet 6 inches grates, and additions of 100 revolutions increased the air-pressure to the extent of about 0.5 inch each.

The number of revolutions required for the Conqueror's 4-feet fans at full power would probably be too great if they had to be worked frequently and for long periods. In other vessels intended to be worked in this way a fan will be placed at each end of each boiler-room, or near the center, as in the Satellite, to equalize as much as possible the draught in all the furnaces. The fans are also to be capable of maintaining an air-pressure equal to 3 inches of water at full power, which will insure a moderate rate of speed for the fans and their engines at all times. In the Conqueror, want of equality in the draught to the fires, owing to the fans being at one end of the boiler-room, was very noticeable, but in this case a better arrangement could not be made, nor could larger fans be employed.

In feeding the low boilers care was required to prevent the water exceeding 2 inches in the guage glasses, or about 6½ inches over the crowns of the fire-boxes. Whenever it rose above this height, slight priming immediately set up. This, however, was probably more apparent than real, for it occasioned no inconvenience at the engines. The water in the high boilers, on the other hand, was perfectly steady, no symptoms of priming being seen at any time, although the water was allowed to rise occasionally as high as two-thirds of the glasses.

In all the vessels referred to, a separate small engine, instead of the usual pumps worked off the main engines, is provided for feeding the boilers, and placed in each engine-room. These draw only from the feed tanks, and supply all the boilers. Their speed is therefore regu-

lated independently of that of the main engines, but in accordance with the wants of the boilers. Each boiler-room is fitted also with a small engine connected with the feed tanks and sea, so that the person in charge of each set of boilers has an independent command of his feed. This arrangement was found to work remarkably well, and proved to be very convenient on the trials, especially in the Satellite. The feed engines having two double-acting pumps, the supply of water to the boilers was very uniform, and it was practically free from air. The pressure-gauges on the delivery pipes showed a variation of not more than 5 pounds, whereas with the ordinary feed pumps a variation to the extent of the boiler pressure at each stroke is commonly observed. This, no doubt, operated in some degree to keep the water steady in the boilers. All the feed-engines as well as the fan-engines in these vessels are arranged to exhaust into the condensers, and also into the atmosphere.

The boilers were in each case filled with fresh water for the purpose of the trials. The best Welsh coal was used, and the stokers were trained men from the Reserves.

It is to be regretted that no estimate can be given of the quantity of coal burnt under this system of forcing the combustion, the trials being made almost entirely with the object of observing the behavior of the boilers, and of realizing the maximum power obtainable; and being in a certain degree progressive, they were necessarily of short duration—too short, in fact, to admit of any account being taken of the rate of combustion of the fuel which could be considered of value. One thing appears certain, viz, that as the air-pressure was advanced, the increase in the consumption of fuel proceeded at a much higher rate than did that of the power given out at the engines.

This is borne out by the observations made of the temperatures produced in the uptakes. On the forced draught trial of the Satellite with the two forward boilers, a pyrometer fixed to the uptake registered a temperature from 1,000° to 1,200° Fahrenheit, whilst on the steam-blast trial of the Heorine a pyrometer, similarly placed, recorded from 775° to 850°. Also, on the trials of the Conqueror, pyrometers were attached to the funnel just above the junction of the uptakes. On the first occasion, under the ordinary draught, the temperature varied from 275° to 280°, when the steam blast was off. When the blast was on, it varied from 490° to 600°. On the trial immediately following, when the fan draught was used, the temperature registered ranged from 850° to 1,000°.

The forced draught trials made in these two vessels can in reality only be regarded in the light of experiments, carried out with the general object of making the ground sure for an extended application of the system, and although they are not so complete as could be desired, they have yielded useful information. The results obtained are sufficient to show that, with engines of suitable size, the steaming power of the lower boilers can, by employing forced draught, be increased by

about 30 per cent. beyond the maximum power hitherto obtainable with the steam blast, and that the increase of effect is even considerably greater than this in the case of the high boilers.

The advantages which follow the successful application of this system of forcing the draught in the boilers of war ships are of considerable importance. It enables the machinery to be constructed within the limits of the space and weight which are sufficient for their ordinary services, and admits of the reserve of power being stored in the light fans and fittings, instead of in the cumbrous boilers and machinery, as heretofore. Many of the existing ships might also, at little cost and in a very short time, have their power and speed materially increased by the addition to their boiler-rooms of the few comparatively light and simple fittings.

Neither in the generation of the steam nor in its employment in the engines is economy to be expected by this method of working the boilers, but neither is it necessary for the few and comparatively short periods that they will probably require to be forced in this manner. The adoption of the fan-draught system has this advantage: It tends to facilitate economical working rather than otherwise under ordinary conditions, because it enables relatively smaller cylinders to be used.

With regard to the endurance of the boilers, there can be no doubt that the frequent use of the forced-draught would produce a great diminution in the life of those parts subjected to the intense heat. But probably, under the conditions that will obtain, this will not be of serious moment. An examination of the boilers of the Satellite and Conqueror after the trials showed that they had not suffered to an unusual extent by the exposure to the intense heat for the short time the trials lasted. Only two or three of the iron tubes were found to be weeping in the low boilers, and in the Conqueror's a few seams and rivets and about twenty of the tubes were leaking slightly.

The machinery of all the vessels above referred to was constructed by Messrs. Humphrys, Tennant & Co., of Deptford, and worked in an eminently satisfactory manner throughout the trials, especially so in the case of the Satellite, the engines of which were tested to a much higher degree than was stipulated for.

Some of the points to which I have made reference in connection with these trials may seem to be of little general importance, but in my opinion they are all of more or less value to engineers who have to provide for the development by fan draught of the extra power required in the first instance and its reproduction on emergencies. It is a question, however, which appears to me to be well worthy of consideration whether the employment of this system of forcing the draught in ordinary boilers could not be extended to other vessels besides war ships.

It would probably not be found advantageous to adopt it generally in vessels which make voyages of longer duration than six hours, or

which run continuously, on account of the delays which cleaning the furnaces and tubes would involve, and the additional wear and tear which would be incurred. But the classes of vessels for which it would appear to be particularly well suited are mail or passenger steamers which make short passages at high speed, occupying about six hours or less, and in which economy of fuel is of minor importance compared with that of keeping good time. There are many services of steamers around our coast in connection with the railway system, such as those which run from Dover, Folkestone, and Newhaven to the French ports, the Dublin and Holyhead boats, and others, to which this is applicable. These boats are mostly paddle-wheel, so that very little, if any, benefit would be gained by supplying the existing vessels with the apparatus for applying air pressure, because the speed of the engines being restricted, the additional steam generated could not be utilized.

In future vessels designed for these services, however, the piston speed could be increased relatively to that required for the wheels, and without adding to the weight of the existing machinery; the additional power obtainable by the fan draught could be made available by increasing the speeds of the ships, thus shortening the time of passage; or, the same speed of vessel could be obtained for a considerably less weight of machinery, if desired.

The merits which the fan-draught system has to recommend it for such purposes are—

That the extra apparatus required is simple, light, and comparatively inexpensive. It admits of the air pressure being applied and removed readily. Fans of sufficient size to run at a moderate speed, having engines of simple construction, are not likely to give trouble if properly attended to. The system is unquestionably effective, seeing that the productive power of ordinary high-pressure boilers can be increased by its agency at least 30 per cent., and probably more. No extra skill is necessary for the manipulation of the apparatus, but a little additional care and attention would probably be required. Provided these be given, there is no reason to suppose, judging from the short experience of its working in the Conqueror, that any serious risk to life or property would be incurred by using the forced draught in connection with high-pressure boilers of the types commonly employed in the mercantile marine. In the class of passenger or mail steamer referred to steam could be raised by the fans to full pressure for starting whilst the passengers are getting on board; thus the inconvenience to which they are frequently subjected by the roaring off of the steam until the boat gets under way would be avoided.

These numerous points are collectively of sufficient importance to entitle such an extension of this system, in my opinion, to consideration.

Natural-draught trials.

	Satellite. Apr. 3, 1882.	Heroine. May 30, 1882.	Hyacinth. July 25, 1882.
Duration of trial .... hours..	4	6	3
Number of boilers used	4	4	4
Mean steam pressure in boilers .... pounds..	84	82.6	81.6
Mean vacuum .... inches..	26.5	26.1	26.0
Mean number of revolutions per minute	98.52	104.2	105.8
Mean pressure { high .... pounds..	32.36	32.07	31.6
Mean pressure { low .... do....	13.85	12.9	14.1
I. H. P. { high	493 } 1,116	515 } 1,127	515 } 1,195
I. H. P. { low	623	612	680
Grate area .... square feet..	110	110	110
I. H. P. per square foot of grate	10.15	10.25	10.87
Tube surface of boilers per I. H. P. .... square feet..	2.18	2.16	2.03
Total heating surface, per I. H. P .... do....	2.61	2.59	2.44
Wind force	4	1 to 4	2 to 4
Sea	Smooth.	Smooth.	Moderate.
Diameter of screw .... feet..	13′ 0″	13′ 0″	13′ 0″
Pitch of screw	13′ 6″	13′ 6″	13′ 6″
Draught of water { forward	11′ 8″	11′ 10″	11′ 8″
Draught of water { aft	14′ 9″	14′ 9″	14′ 9″
Maximum temperature in the boiler-rooms	111°	108°	98°
Mean temperature in the boiler-rooms	90°	87°	84°

Steam-blast trials.

	Heroine. May 31, 1882.	Hyacinth. July 25, 1882.
Duration of trial .... hours..	3	2
Number of boilers used	2	4
Mean steam pressure in boilers .... pounds..	83.9	85.0
Mean vacuum .... inches..	26.0	25.1
Mean revolutions per minute	89.4	111.6
Mean pressures { high .... pounds..	22.45	34.05
Mean pressures { low .... do...	9.63	16.9
I. H. P. { high	310 } 702	586 } 1,445
I. H. P. { low	392	859
Grate area .... square feet..	55	110
I. H. P per square foot of grate	12.76	13.1
Tube surface per I. H. P .... square feet..	1.73	1.68
Total heating surface per I. H. P .... do....	2.08	2.02
Wind force	1	2 to 4
Sea	Smooth.	Moderate.
Area of funnel .... square feet..	*7.5	15
Blast nozzles:		
Number	2	4
Diameter .... inch..	$\frac{9}{16}$	$\frac{9}{16}$
Diameter of screw .... feet..	13′ 0″	13′ 0″
Pitch of screw	13′ 6″	13′ 6″
Maximum temperature in the boiler-rooms	98°	100°
Mean temperature in the boiler-rooms	87°	88°

* A diaphragm was placed in the funnel for this trial.

Forced-draught trials.

SATELLITE.

	May 10, 1882.	July 5, 1882.	July 11, 1882.	July 11, 1882.	July 11, 1882.
Number of boilers used	2	4	3	3	3
Duration of trial .... hours..	3	½	½	½	2
Air pressure .. inches of water..	1 to 1½	¼	½	1	1¼ to 2
Mean steam pressure in engine room .... pounds..	79	90	86.5	78.7	80.5
Mean vacuum .... inches..	26.6	25.0	25.38	23.88	23.41
Mean revolutions per minute..	95.4	113.5	103.36	110.66	121.45
Mean pressure { high . pounds.	27.52	29.95	29.4	33.7	31.93
Mean pressure { low .... do ...	10.58	12.52	12.85	14.9	14.45
I. H. P. { high	405 } 864	524 } 1,171	469 } 1,074	575 } 1,326	598 } 1,397
I. H. P. { low	459	647	605	751	799
Area of fire-grate .. square feet..	55	110	82.5	82.5	82.5
I. H. P. per square foot of grate	15.7	10.6	13.0	16.0	16.9
Tube surface per I. H. P., square feet	1.41	2.08	1.70	1.37	1.30
Total heating surface per I. H. P .... square feet..	1.69	2.5	2.04	1.65	1.56
Area of funnel ... square feet..	15	15	15	15	15
Maximum temperature in the boiler-rooms	86°	....	....	....	....
Mean temperature in the boiler-rooms	76°	....	....	....	....

Steam trials.

CONQUEROR.

	January 24, 1883.	January 24, 1883.	January 26, 1883.	January 26 1883.
Number of boilers used	8	8	4	4
Duration of trial .... hours	2	1½	1½	1½
Air pressure .... inches of water	....	1⅛	1¼	1¾
Mean steam pressure in engine-room, pounds	64	65	61	63
Mean vacuum .... inches	28.4	27.6	27.8	27.5
Mean revolutions per minute	100.8	106	92.21	95.2
Mean pressure { high .... pounds	27.7	27.3	24.27	25.61
Mean pressure { low .... do	8.27	11.58	6.98	7.48
I. H. P. { high	2,325 } 4,658	2,408 } 5,842	1,864 } 3,665	2,030 } 4,023
I. H. P. { low	2,333 }	3,434 }	1,801 }	1,993 }
Area of fire-grate .... square feet	585	585	300	300
I. H. P. per square foot of grate	7.96	10	12.22	13.41
Tube surface per I. H. P. .... square feet	2.37	1.89	1.55	1.41
Total heating surface per I. H. P .... do	2.86	2.28	1.88	1.71
Area of funnel .... do	64	64	64	64
Maximum temperature in the boiler-rooms	100°	102°	110°	116°
Mean temperature in the boiler-rooms	73°	85°	91°	98°

Particulars of the machinery of Satellite and Conqueror.

Description of engines—Satellite: Horizontal compound single-screw, return connecting rod, 2 cylinders. Conqueror: Vertical compound twin-screw, direct acting, 3 cylinders to each set.

Diameter of cylinders—Satellite: H. P., 36″; L. P., 62″. Conqueror: H. P., 54″; two L. P., each 70″.

Length of stroke—Satellite: 2′ 6″. Conquerer: 3′.

Diameter of crank shaft—Satellite: 9½″. Conqueror: 13¾″.

Diameter of propeller shaft—Satellite: 8½″. Conqueror: 12¾″.

Crank pins, diameter × length—Satellite: 10″ × 11″. Conqueror: 13¾″ × 12″.

Total length of main bearings—Satellite: 5 feet. Conqueror: 8 feet each set.

Material of shafts—Satellite: Siemens-Martin steel. Conqueror: Wrought iron.

Cooling surface of condensers—Satellite: 1,900 square feet. Conqueror: 9,000 square feet.

Air pumps, number diameter × stroke—Satellite: One, 14″ × 2′ 6″; double acting. Conqueror: Eight, 14″ × 3′; single acting.

Description of screws—Satellite: Two-bladed Griffiths', feathering on Bevis' plan. Conqueror: Four-bladed modified Griffiths'.

Diameter × pitch of screws—Satellite: 13′ × 13′ 6″. Conqueror: 14′ × 16′ 6″.

Number and description of boilers—Satellite: Four of long circular type. Conqueror: Eight of high type, 6 flat-sided, 2 circular.

Working load of boilers—Satellite: 90 pounds. Conqueror: 70 pounds.

Width × length × height of boilers—Satellite: 7′ 5″ diameter × 14′ 8″ long. Conqueror: Six, 12′ 2″ × 9′ 6″ × 15′; two, 12′ 4″ diameter × 9′ 6″ long.

Thickness of shell plates of boilers—Satellite: $\frac{11}{16}$″. Conqueror: $\frac{15}{16}$″.

Material of boilers—Satellite: Siemens-Martin steel, except furnaces, combustion chambers, and tube plates. Conqueror: Siemens-Martin steel, except furnaces, combustion chambers, and tube plates.

Number of furnaces in each boiler and dimensions—Satellite: Two, 2′ 9″ diameter × 5′ long. Conqueror: Three, 3′ 4″ diameter × 7′ 6″ long-high; 3′ diameter × 7′ 6″ long circular.

Total grate area—Satellite: 110 square feet. Conqueror: 585 square feet.

Tubes, material of, and dimensions—Satellite: Wrought iron, 0.165″ thick, 2½″ diameter, × 5′ 8″ between tube plates. Conqueror: Brass, 0.137″ thick, 3″ diameter × 6′ 6″ between tube plates.

Total tube surface of boilers—Satellite: 2,430 square feet. Conqueror: 11,050 square feet.

Total heating surface of boilers—Satellite: 2,920 square feet. Conqueror: 13,340 square feet.

Funnel, description and height from dead plates of lowest furnaces—Satellite: Lifting, 50′. Conqueror: Fixed, 65′.

Fans for forced draught—Satellite: Two, 5′ diameter. Conqueror: Four, 4′ aft; two 5′ forward.

Description and dimensions of fan engines—Satellite: Direct acting, single cylinder, 7″ cylinder × 4″ stroke. Conqueror: Brotherhood, 3 cylinder, 7″ diameter × 4½″ stroke.

DISCUSSION.

Mr. F. MARSHALL. My lord president and gentlemen, I have read the paper, or have listened to its reading, with very great interest. The experiments recorded (for, as Mr. Butler says, I think we must only regard them as experiments) are certainly of the most interesting nature, and promise to be of very great importance in the future development of marine engineering. The only direction in which we can go with a view to increasing power with the present size of engines, or keeping the engines smaller in order that we may reduce weights, seems to me to be the direction which I ventured to point out at the meeting of the Institution of Mechanical Engineers in Newcastle, and which has been adopted since very generally. I mean the system of forced draught and high speed of movement adopted by Mr. Thornycroft in the working of his torpedo vessels, and which I had the honor of carrying out in designing the machinery for the Chinese cruisers constructed by Sir W. G. Armstrong & Co. There are great difficulties in working this system, and I think Mr. Butler in his paper has pointed them out, or has indicated them at least. These, of course, are really experiments. It appears to me that in carrying out the experiments much too large grates have been adopted for the work to be done; that had smaller grates been adopted a larger and more satisfactory result would have been obtained. We have no records here of the quantity of fuel burned—that, of course, would have told us whether the grate were large or small in proportion to fuel burnt—but we have a comparison between the grate area and the indicated horse-power. Now I need not tell you, my lord president or gentlemen here, that to work a 7-ft. 6-in. grate efficiently under such conditions as men have to work in forced draught is a physical impossibility. The back ends of the bars must be almost entirely open, and a very large amount of the air pass off, with the effect of reducing the efficiency of the gases, and cooling the boilers. Therefore, I think in the case of the experiments here the maximum effect would not by any means be obtained. The grate areas, I think, were much too large. I speak from my own experience. We had very great difficulty in keeping even 6-ft. bars properly covered in the trials to which I allude, conducted in the north of England. There is one very impor-

tant matter brought out by this paper. It has been a generally received opinion among engineers that the low type of boiler was more efficient than the high type. We are told this morning, and with apparent truth, that the high boilers show very little, if any, inferiority to the low boilers. There is one question I should like to ask Mr. Butler, and that is, whether in putting it at 10 per cent., as he gives it here, of superiority of the low boilers of the Satellite over those of the Conqueror, he has considered the influence of the pressure. I presume in estimating the efficiency he was working from the indicated horse-power developed. He says they were nearly 10 per cent. better, but it is important to know whether that 10 per cent. was estimated on the pressure of 90 pounds or on the pressure of 70 pounds, the engines of the Satellite having a working pressure of 20 pounds higher than those of the Conqueror. That would, of course, fully account for the difference in the efficiency of the boilers, that is, it would place them almost on a level. There is one point against that conclusion, viz, the Satellite's boilers have iron tubes, while those of the Conqueror are, I understand, of brass. That circumstance would rather speak in favor of the Conqueror, the brass tubes being more efficient than the iron tubes. There is one thing more I would remark. It appears that the maximum amount of power got from the square foot of grate here is about 16½ indicated horse-power. Now, from experiments I have recently conducted with the locomotive type of boiler, I believe there is no reason why we should not obtain 20 indicated horse-power from a square foot of grate. The difficulty of priming seems to have been experienced to a greater extent in the low boiler than in the high boiler. That I cannot quite account for and can scarcely understand, because the difficulty has generally been to keep the water uniform in boilers of the high class on high-speed trials, whereas there has generally been no difficulty with the low type. I may say that in the long series of trials which we had in the three cruisers, extending, I think, over something like twenty high-speed trials, lasting from four to eight hours under forced draught, we never were in the least degree troubled with priming, and we generally worked at a pressure of about one inch. Referring again to the question of the high and the low boilers, there is one point in reference to the efficiency of the high boiler under forced draught which is worthy of notice, I think. It will be noticed that in both the natural-draught and the steam-blast trials, which depend, of course, upon the efficiency of the funnel, the temperature of the gases was higher in the funnel in both cases in the Satellite than it was in the Conqueror. This I think we may look for from the mere fact that the gases have a more direct flow through the low boilers than they have through the high, and of course, on the other hand, where the draught is given by fans, we have the lower parts of the boiler under a much more intense heat. In the Conqueror boilers, the furnace and back flame-box capacity is greater than in those of the Satellite, and present a larger absorbing surface to the intense heat developed in those

parts; and further, the resistance to the direct flow of the gases presented by the high boiler as compared with the low, will, I think, account for what we had not expected, namely, the apparently much higher efficiency of that class of boiler when working under forced draught. I should like to observe on this point that the greater efficiency of the heat in the Conqueror's boilers over those of the Satellite might be balanced in the latter by an increased length of run. It is to be distinctly observed, in speaking of the economy of fuel in connection with forced draught, that everything depends upon the relation of the grate area to the heating surface. Now, in both the Conqueror and the Satellite that relation was as low as 1 to 26; I think in the case of the Satellite it was 1 to 23. That is a very low ratio, in fact, it is a natural-draught ratio or thereabouts; whereas in the cruisers to which I have referred that relation was about 36 to 1. In the case of locomotives, which may be regarded as our standard in this matter, the relation is about 1 to 80. Now, I need not say that in future the locomotive seems to be the form of boiler to which we must look if we are to have small weight for large power. So far as my own experience goes, there is no reason why the locomotive boiler should not be the boiler of the future for, at any rate, war vessels. I have recently made some very excellent experiments with boilers we are now constructing for the Danish Government, in which we have got very high results without the slightest evidence of priming, the steam being blown off under very disadvantageous circumstances. It is quite clear that the most important matter of all to us is that we should be able to reduce our grate areas to the smallest, in order that we may keep them well covered, use higher pressure of air and increased efficiency of fuel. Mr. Butler in his paper mentions that we cannot look for highly economical results from forced draught. I beg to differ from Mr. Butler in that respect. We have in the case of the locomotive generally an indicated horse-power for about $2\frac{1}{2}$ pounds of coal. That is not a very highly economic result; at the same time it is a favorable result, and that I think we obtain almost entirely from having small grates and a large amount of absorbing surface and a high air-pressure under the grate. The absorbing surface being in the tubes is got with a very small amount of weight. As to the boilers of the Conqueror, if you will pardon my saying so, the boilers appear to be about one-third too large for the engines. That, of course, is all right when we are working, as in this case we were working, under ordinary conditions. The engines also seem to have developed very far short of the power which was possible to them. I feel sure that under similar conditions to those used in the cruisers to which I have referred, at least 8,000 indicated horse-power could have been obtained from those engines. That is confirmed by the fact that when working at half power 4,000 indicated horse-power was obtained. I beg to express my thanks, and I am sure the Institution will do so also, to Mr. Butler, for his very valuable paper, and also I think we

may say to the Lords of the Admiralty, for having permitted experiments of such very great value to be laid before this Institution.

Mr. RAVENHILL. I should like to ask the writer of this paper a question or two. He was rather pressed for time, and he omitted one or two paragraphs at the end of his paper, notably one where he alludes to the possibility of great improvement in some of our mail-boats, where the distances from port to port are but short, if forced draught could be introduced. Now, at Dover, some years ago, fans were fitted to some of the mail-boats there, the object sought being to enable those vessels under all conditions of weather—that is, whether the wind was off the shore or blowing into the harbor—to start carrying their full pressure of steam. One of the first things we did there was to run all our furnace bars together, and the trials were abandoned. I would like him to tell us, if he can, the condition of the fire-bars after the trials on board the Conqueror and on board the Satellite, because in the paper he quotes very high temperatures indeed as having been registered in the uptakes of the boilers on both those vessels. There is yet, no doubt, a great deal to be learnt, and probably the principal point to be learnt will be found to be how the boiler will stand the increased work to which it will be subjected. The advantages, of course, if it could be carried out on short distance stations would be very great indeed, because the difference between the ordinary draught and the forced draught, as described by the writer of the paper, say on the Folkestone station, would produce something very like an increase of a knot an hour in the speed of the fastest vessels on that station. This would be an enormous gain, and anything that Mr. Butler can supplement this paper with, with reference to observations made on the condition of the boilers at the conclusion of the trials, would add vastly to its interest. I am quite sure the Institution owes to him and to the Admiralty a deep debt of gratitude for having given us these their first experiments. It has been suggested that I should ask Mr. Butler what state the uptakes and the chimneys were found to be in at the conclusion of the trials after they had been subjected to this very high temperature of, say, 1,000 to 1,200 degrees Fahrenheit.

Mr. ROBERT HUMPHRYS. My lord, I do not agree with Mr. Marshall or Mr. Butler in the statement that a 7-ft. bar is too long to be worked efficiently. In the case of the Conqueror it was 7 feet 6 inches, and on the trial we certainly had fluctuations in the air-pressure in the stoke-holes, but in my opinion that was due to the fact of the difficulty we had in making the stokers realize that they were burning 40 per cent. more coal than they usually did with the steam blast, but towards the end of the trial the bars were thoroughly well covered, and we were getting the full effect from them. Mr. Marshall states, also, that he thinks that we should reduce our grate areas to the smallest, in order that we may use high air-pressure. I venture to differ entirely with him there, because if you work with high air-pressure it is more difficult to man-

age, and your bars get dirtier sooner, because they burn a greater quantity of coal in a given time. Mr. Marshall has also told us that he has obtained in the locomotive boiler 20 horse-power per foot of bar. I have no doubt a great many members here will be very glad if he will tell us under what circumstances he did that. Unfortunately, we have had some experience of locomotive boilers, and we have not exactly been able to do it. Making experiments in one's own yard and making experiments on board ship I find are totally different things. Captain Noel in his paper read yesterday stated that he was afraid that inclosed stoke-holes would lead to a scare amongst the stokers if any sudden rush of water were to come in. I would like to point out that the arrangement carried out in the Conqueror is really safer than an ordinary stoke-hole, because men could get out by proper steps fitted on the bunkers through the doors in the covering plates, and then through the double doors of the engine-room. In the event of steam-pipe or boilers being damaged the men would be practically safe, because they have an outlet into the engine-rooms through the double doors. Mr. Butler comes to the conclusion that as the air-pressure increases the consumption of fuel proceeds at a much higher rate than that of the power given to the engines. Consequently, I think his ideas would coincide with mine, that the better plan would be to lower the pressure.

Mr. R. H. Andrews. My lord, I think it will be very interesting to the meeting, if Mr. Butler will kindly tell us what was about the thickness of the fires in the Satellite's and Conqueror's boilers during these trials. We are inclined to associate the results of these experiments with those of the locomotive type of boilers, where we find the fires varying in thickness from 14 inches to 2 feet, and in some cases more than that. When working with forced draught, it is absolutely necessary to have the fires much thicker than with ordinary (natural) draught. The Conqueror's furnaces are only about 3 feet 4 inches diameter, and assuming the dead plate to be about two inches above the center, it only leaves about 1 foot 6 inches from the top of the bars in front to the top of the furnace, and if there is a thickness of fire of only 1 foot at the beginning of the bars, which is practically at the mouth of the furnace, how is it possible for the stokers to put back coal enough at the end of 7-feet bars to keep them well covered? I must, therefore, beg to differ from Mr. Humphrys that these 7-feet bars can be kept sufficiently well covered for working with forced draught. Long bars cannot be so efficiently fired as short ones, and I find, from many years' experience with torpedo-boat boilers, that it is practically impossible to fire efficiently bars more than 5 feet 6 inches long, that is, to maintain a thickness of fire up to 14 inches, and that a stoker has quite as much as he can do to fire a furnace of 25 square feet in area, when burning 80 to 90 pounds of coal per square foot of fire-grate per hour, to keep the bars well and evenly covered; and if they are not kept well covered I am sorry to say we know too well that the tubes will soon begin

to leak. This is the case with the locomotive type of boiler, and probably would be also with the Satellite's, where the flame impinges directly on to the mouths of the tubes. I think that in designing boilers of the Satellite's and Conqueror's type, to be worked under forced draught, we must go in for much larger furnaces, say 4 feet in diameter. This will cause a corresponding reduction in the length of the bars, and admit of their being fired efficiently. It will also be interesting to the meeting to know what was the pressure of air in the furnace, in the combustion chamber, in the smoke-box, and at the root of the funnel in each ship, so as to compare the loss of pressure due to resistance in each type of boiler. The quantity of coal burnt in each type of boiler cannot be compared by the pressure of air in the stokehole, as the pressure depends upon the resistence the air meets with in passing from one part of the boiler to another. In the Satellite's boilers the products of consumption go almost direct into the tubes from the furnace, and from the smoke-box straight up the funnel. This is similar to that of the locomotive type, but in the Conqueror's boilers the gases have to turn up from the furnace into the combustion chamber, then turn again at right angles into the tubes, and a third turn from the smoke-box into the funnel. We should, therefore, expect to find a great difference of air pressure in the stoke-holes of these two ships when burning equal quantities of coal per square foot of grate per hour in the same sized furnace, and, consequently, the air pressure is no measure of the consumption of coal; nor can the consumption be compared by the air pressures used in different types of boilers, only in boilers of the same or similar types. I find with the ordinary locomotive type of boiler in torpedo-boats, that after steaming, say an hour, at full speed, the air pressure increases considerably, but there is no more coal being burnt then than at the beginning; it is simply due to the increased resistance to the air and gases owing to the fire-bars, tubes, &c., getting dirty and partially choked. I would like to call attention to one thing in connection with this paper. We have been speaking of the "consumption of coal," but I think it would be far better to call it the amount of "coal expended." It is well known that we do not get so much benefit out of the coal used as we ought to when working under forced draught, and that in boilers of the locomotive and Satellite type a great quantity of coal is blown into the smoke-box and up the funnel in a partially consumed state, and at present there does not appear to be any means of avoiding this waste. I therefore think that under such conditions "coal expended" is a much more appropriate term than "coal consumed."

Mr. J. WRIGHT. My lord, will you allow me just to say a few words before you close the discussion? Attention has been called to the very good behavior of the high boilers in the Conqueror as compared with the low boilers in the Satellite, and I think if Mr. Marshall will look at the drawing, where the boilers are not shown in detail, he will see as reasons for this that there is a very large amount of steam space, and the water

spaces are also very good. Another thing Mr. Butler mentioned in his paper, although he did not read it, was that the feeding in the Conqueror was done by a separate feed-engine, and that the feeding was remarkably regular. We know that feeding from the main feed-pumps is very often irregular and spasmodic when working at high powers, and very often leads to priming. In this case the feeding was everything that could be desired. I am afraid that Mr. Humphrys is very much in the minority with regard to length of fire-bars. When the Conqueror's bars were shortened to 6 feet 6 inches, the results obtained were certainly, I think, better than with the longer bars, and the increase of air pressure that may be required to be given to fire-bars of moderate length that can be well worked would be very little. I am glad Mr. Humphrys has called attention to the comparative safety of the inclosed stoke-hole. It is a point that Captain Noel alluded to yesterday as a very serious matter, but I think that when men once get accustomed to it they will really prefer working in a closed stoke-hole to an open one. It is certainly more comfortable, if you do not stand immediately under the fans; it is cooler and more comfortable in every way. Mr. Butler mentions in his paper that our neighbors, the French, have been ahead of us in this matter of applying forced combustion in closed stoke-holes to ordinary boilers. The experiments that have been made here as yet are, in a measure, preliminary, and only to a certain extent of value. They are not of much value to the commercial marine at the present time. They would have been more so, I dare say, if we could have had given the coal consumption, but I am afraid the effect of that would have been, upon those, at all events, who saw the rapid rate at which the coal went, rather to deter them from adopting the system. What I was going to say was this, that our good neighbors, the French, having gone ahead of us in this matter, we should be very glad if our distinguished member, M. de Bussy, would favor the institution, at some future time, with some results of their trials under forced combustion.

Mr. BUTLER. There are not many points, my lord I think, I have to allude to, and I will not detain the meeting long. Mr. Marshall spoke of the long grates of the Conqueror's boiler being, in his opinion, unworkable. I must say I agree with him, and I must differ from Mr. Humphrys in that respect. I was present on the trials made in the Conqueror, when these long grates were fired, or attempted to be fired—because it cannot be said that they were fired efficiently when every now and then a rake had to be put in to the furnaces to level the fires. The furnace doors had to be kept open for some considerable time, and thus large volumes of cold air at considerable pressure were driven straight in onto the backs of the tubes. With regard to the priming that I made reference to as being observed in the boilers of the Satellite, I endeavored to make it clear that this was probably only apparent because no water was observed to come over into the engine-room; but the effect it had on the people in the boiler-

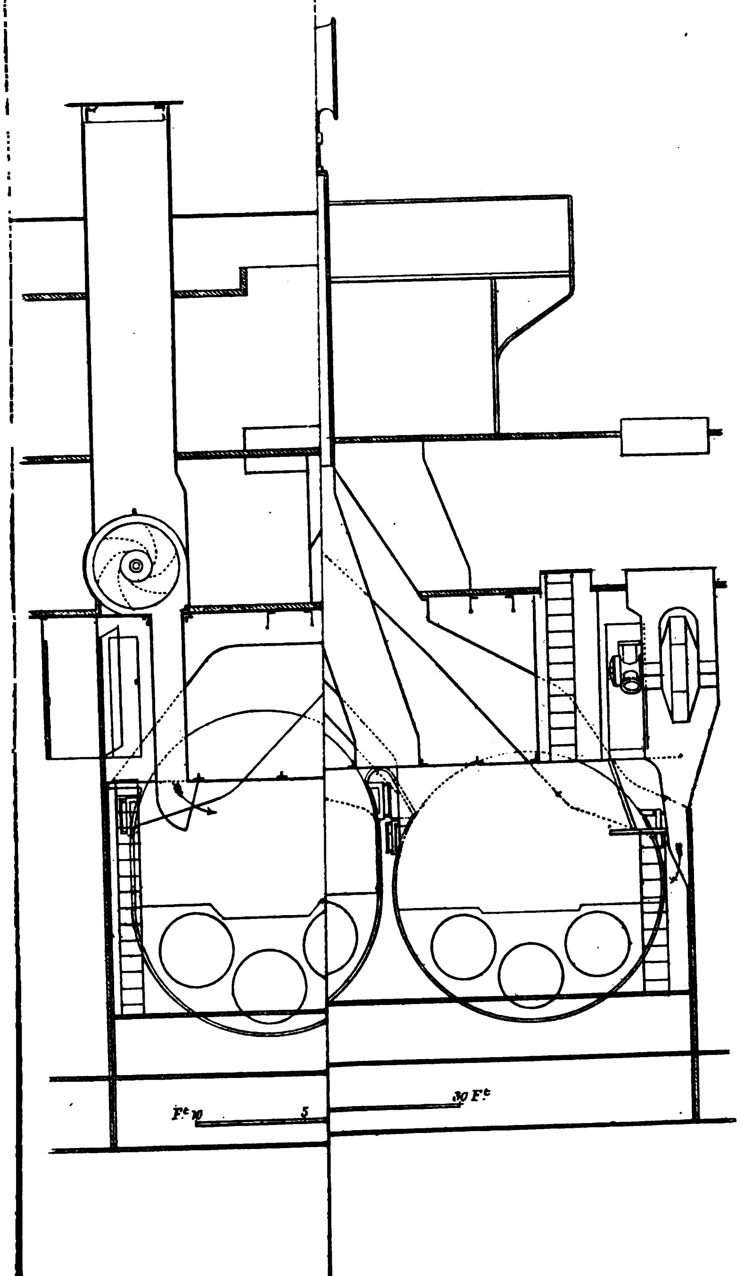
room—the men in charge—was just the same as if it were real and the water had come over, because the disappearance out of the glasses rendered it necessary for safety's sake for the feed-engines to be put on to make up the apparent deficiency. With regard to the Conqueror's boilers, it must be recollected that they were designed to produce steam for the engines only sufficient for the contract power when worked with the steam blast, and therefore, when they are worked with the air pressure, they appear to be extremely large in proportion to the size of the engines. Then, as for the question of lengthening the tubes in these boilers, so as to get a better result and a less temperature in the uptakes, it must be remembered that the boilers of these ships were, and for war ships must necessarily be, designed primarily to enable them to meet the requirements of the ordinary work of the ship at all times, and the application of the forced draught in them will be very exceptional; in fact, it is not intended that the forced draught shall be applied except in cases of emergency or occasionally for a short time to enable the men to be trained in its use. Mr. Ravenhill asked what condition the fire-bars of the Conqueror and Satellite were in after these trials, and also the uptakes and chimneys. In the Satellite, in fact in both these vessels, the fire-bars are made of wrought iron, about 3½ inches deep. No effect was observable on the fire-bars at all, but the fire-bridges of the Satellite were found to be slightly burned; no ill effect was observed on the uptakes and chimneys of either of these vessels. Mr. Andrews wished to know what thickness of fires we kept in the Satellite and the Conquerer. In the Satellite, so far as I can recollect, it was about 7 inches, and in the Conqueror we attempted to keep 10 inches, and no doubt 10 inches were kept on the front bars, but it is very doubtful whether that thickness was maintained uniformly. I do not think there was any other point, my lord, that called for any remark from me. I beg to thank the meeting most sincerely for the manner in which my paper has been received.

Mr. W. H. White. Will your lordship allow me for a moment? M. de Bussy will not rise himself, although I have been asking him to do so, but he asks me to be his mouthpiece, and to say that he will have the greatest pleasure in furnishing the institution with the results of the best French experience which he can put at the service of the institution.

The President. Gentlemen, I am quite sure you will allow me to express a double vote of thanks on this occasion; first of all to the writer of this most interesting, and, I venture to think, one of the most important papers which is likely to be read—to Mr. Butler, for the reading of that paper, and to our distinguished and honored friend, M. de Bussy, for the last announcement that he has made. I dare say you will pardon me a moment if I make an observation or two upon the discussion. I regret extremely, gentlemen, and I have no doubt many of you will regret with me, that no estimate has been given, apparently for some very good

reasons, of the quantity of coal burned, because really (I speak with some little knowledge upon this subject, because I live in a smoke country, and we all know that smoke is practically wasted fuel, and those who live in a smoke country know what they suffer in consequence) economy of fuel is the great object that we are all seeking to obtain by the introduction of improved boilers and furnaces. In the long voyages it becomes a question of enormous magnitude, and the saving of fuel is really one of the greatest questions at the present day in that view. There is another point, and that is this, that all the study of all our highly instructed engineers is of very little use, and is really thrown away in the improvement of this machinery, if those who have to use it are unable to use it properly. And that leads me to the question of stoking, which is one of the most important questions of all. How we are to secure at all times in the navy not only a sufficient supply but a reserve of highly-trained stokers, is really one of the most important questions that can be dealt with as a national question. I merely wished to touch upon this subject, and I am very much obliged to Mr. Andrews for that important remark which struck me at the time when he called attention to the question of smoke, which, as I said before, in practice really means wasted fuel. I am sure you will allow me to return jointly to Mr. Butler and M. de Bussy your united thanks.

nd Conqueror under F



XII.

THE ADVANTAGES OF INCREASED PROPORTION OF BEAM TO LENGTH IN STEAMSHIPS.

By J. H. BILES, Esq., *Member*.

[Read at the twenty-fourth session of the Institution of Naval Architects, 15th March, 1883; the Right Hon. the EARL OF RAVENSWORTH, president, in the chair.]

My object in reading this paper before this institution is to give some of the advantages which have actually been obtained by adopting increased proportions of beam to length in some steamers which have been built recently by Messrs. J. & G. Thomson, of Glasgow. This firm have for some time past made it a rule, in tendering for any ship of large size or high speed, to suggest an alternative design to that proposed by the ship-owner, and the modification has invariably taken the form of increased proportion of beam to length. In some cases the proposed modifications have been adopted, and the actual results of some of these ships are now laid before this institution.

It will be remembered that it was the late Mr. Froude, in his paper on "Comparative resistances of some long merchant ships," who first brought prominently into notice the fact that it was possible, by increasing the beam and by fining the ends to get a form which, at high speeds, would have considerably less resistance than the ordinary merchant-ship form with ten beams in the length. The alternatives which he gave were, however, exceedingly fine-ended, and they did not appear to possess very much advantage at moderate speeds. Consequently they have not been much adopted, but since the reading of that paper there has been less tendency to look upon an increase of beam as likely to reduce speed, though there has been not much notice taken of the fact that this increase should be associated with an increase of fineness in the ends. It is necessary to note, therefore, in all that follows, that whenever an increase in the proportion of beam to length is spoken of, it is assumed that it is accompanied by an increase of fineness in the form.

Increased proportion of beam to length gives:

Increased speed.—It is very difficult to deduce really reliable information as to the relative merits of different forms of ships from the results of their steam trials. The efficiency of the means of propulsion is a

large element in the gross result, and it is not possible in the present state of our knowledge to take a proper account of this efficiency. The only possible method of comparison is to choose ships somewhat similar in size, speed, and elements of propeller, and from the results of their steam trials deduce the relative efficiencies of the form by means of the Admiralty formula, or some similar one, which we know for small variations cannot be very far wrong. The comparative results cannot, however, be considered as accurate, but will give some rough guide to the relative merits of different forms.

The following are the particulars of two vessels built by Messrs. J. & G. Thomson. They are mail steamers, carrying cargo of such a nature that internal capacity is of more importance than weight-carrying:

	Length.	Breadth.	Draft on trial.	Block coefficient.	Prismatic coefficient.	Kirk's angles.	Indicated horse-power.				Admiralty displacement constants.			
							15 knots.	14 knots.	13 knots.	12 knots.	15 knots.	14 knots.	13 knots.	12 knots.
A...	390	42	18.2	.646	.694	9°20′	5,130	3,390	2,280	1,660	216	256	301	324
B...	375	45	20.3	.598	.651	9°5′	3,940	2,860	2,280	1,810	280	311	315	316

These two cases I submit to this institution as a direct proof that the broad but fine-ended steamship B is a more economical form to drive than the larger, narrower, and fuller-ended form A.

As may be seen by the coefficients, they are both fine ships, and therefore the comparison is, in my opinion, a fair and a typical one. The displacement on trial was 5,500 tons in A and 5,900 in B. The particulars of the propellers are:

A.	Diameter.	Pitch.	Actual surface.	Revolutions at 15 knots.	Slip per cent.
A	18.6	28.6	102	63.6	15.8
B	18.0	28.0	112	63.4	14.5

These propellers are both of comparatively small diameter for the power developed, but it is not necessary to state here why they were made so small. They are not very dissimilar, except in the surface, which is 10 per cent. greater in the case of B than A. As well as I have been able to determine from the data at my disposal, it appears that within certain limits a considerable alteration may be made in the surface of a propeller, and the result obtained may not be much altered in smooth-water speeds. The forms of the two propellers are very similar. Whatever advantage there is in diameter will most probably be in favor of the propeller of A. Whatever difference there may be, cannot be great compared with the whole difference in the results. The only other difference which may have affected the result is the state of the bottom. A had been launched five months before trial, and her

bottom had not been painted again below the 10-feet water-line. B had been docked three weeks before her trial. This, however, could not have caused much loss of speed, as we have always found that vessels which have been lying in Messrs. Thomsons' wet dock, when they have gone into dry dock, have been practically as clean as when launched. Further, we have tried several vessels on the Clyde before docking, and they have been tried at Stokes Bay immediately after docking, and generally with very little difference in the result, the greatest discrepancy amounting to 6½ per cent.

The Admiralty constants show that the vessel B was nearly 30 per cent. better than A. In further confirmation of the superiority of the broader ships, the following two cases are given:

	Length.	Breadth.	Molded draught.	Block coefficient.	Prismatic coefficient.	Kirk's angles.	Indicated horse-power at 14 knots.	Admiralty constant at 14 knots.
C............	365	43	17.1	.588	.656	8.37	2,910	257
D............	365	45¼	17.4	.513	.624	8.41	2,740	282

In these cases there is a difference of 10 per cent. in favor of the broader ship. These vessels had lain about the same time in the water after launching, and had not been docked previous to their trials. Their propellers are very similar, being as follows:

	Diameter.	Pitch.	Actual surface.	Revolutions at 14 knots.
C..	17.6	25.6	95	61.8
D..	17.6	25.6	90	61.5

Though there may be some reason for doubting the whole of the advantage in the case of A and B, there appears to be quite sufficient to warrant one in saying that increased proportion of beam to length gives greater speed when it is combined with finer ends.

The displacements of A and B at 24 feet molded draught are 7,690 tons and 7,240 tons respectively. Assuming that the total weight of hull and machinery are the same in the two cases, it will be necessary to increase the dimensions of B to length 386 and the breadth to 46.4, in order that the two ships may carry the same total dead-weight of coals and cargo on commencing a voyage. From data obtained from ships built to Lloyd's three-deck rules it appears that the vessel B enlarged would not exceed A in the weight of hull more than 1 per cent., the depth being the same.

The difference in first cost in the two vessels, A and B, as enlarged, would not be more than 5 per cent., but the difference in horse-power, and therefore in coal consumption, for the same speed would be at least 10 per cent. Assuming that a vessel of this class cost £90,000, and that the machinery cost about £40,000, the extra first cost on the vessel

will be about £4,500, and the decreased first cost on the machinery about £4,000, or a total increased first cost of £500 for a decreased consumption of fuel of 10 per cent., which is equal to at least £1,000 per annum. This is assuming that the same total weight of coals and cargo be carried as in A, but obviously a decreased coal consumption means an increased cargo-carrying on the same draught. The actual dimensions which would give the same cargo-carrying in the two types, after providing sufficient coal for the voyage, must depend upon the length of the voyage, so that each case must be decided according to its special circumstances.

Increased proportion of beam to length allows of *increased depth for the same amount of initial stability.* To show the difference in initial stability due to the position of the metacenter, the curves in Fig. 1 (Plate I) are referred to. From these it will be seen that for the same position of C G, the metacentric height in the light condition may be increased by about 1.1 feet in passing from A to B, and .6 feet in passing from C to D. To a ship which has a negative metacentric height of over 2 feet in the light condition, as several large ships trading to London have, this addition would be of the greatest value in the successful working of the ship in dock. If, however, the stability in the narrow type has been found to be sufficient, a considerable addition may be made to the depth of the vessel when the proportion of beam to length is increased. The advantages due to this extra depth are:

Increased strength due to the increase in the depth of the virtual girder which the ship forms.

Increased finite stability, on account of greater freeboard.

Increased surplus buoyancy, on account of greater freeboard.

Increased internal capacity for cargo.

If the cargoes are generally dead-weight, this increased capacity will be useless for cargo purposes, and will be costly for maintenance, on account of the extra tonnage dues which must be paid. If, on the other hand, the cargoes are light in density, the extra capacity will be of value, and if the increase of proportion of beam to length be made with the increase of depth, a point must be reached where limitation of amount of cargo which can be carried will be the same as in a dead-weight trade, viz, draught of water and freeboard, and in that way all cargoes may be reduced to the same basis as a dead-weight one. One consequence of this will be, that the saving of weight which follows the adoption of steel will be as remunerative in a ship carrying light as in one carrying heavy goods.

It may be remarked, however, that generally an increase of depth is not a necessity of increased proportion of beam to length, as few ships have too much initial stability, but that if an increase of beam gives too much initial stability it is easy to reduce this stability by increasing the depth of the ship.

Increased proportion of beam to length gives of itself *increased strength to the ship longitudinally* by—

(1) Generally reducing the absolute length of the ship, and thus reducing the straining action due to length.

(2) Increasing the breadth of the decks and the bottom amidships, at which points the ship receives the greatest straining action. This is most increased in the decks, as the openings in the deck are not increased with the beam and consequently the increased breadth is put in where the structure is not cut. For instance, in A the upper deck, less machinery openings, is 26 feet wide amidships; in B it is 29 feet, or 11 per cent. more, whilst the beam is only 7 per cent. more.

The section of A and B to Lloyd's scantlings for three-deck ships built of steel gives a stress of 9.5 tons and 4.8 tons respectively per square inch on the upper deck, on the assumption that

$$\text{Bending moment} = \frac{\text{Displacement} \times \text{Length.}}{30}$$

Increased proportion of beam to length gives of itself *increased range and amount of stability*. The curves of stability of A and B are given in Fig. 2 (Plate I), on the assumption that the molded depth and the vertical position of the center of gravity are the same in both cases.

Fig. 3 (Plate I) shows the longitudinal distribution of buoyancy in the vessels A and B at the mean draught on trial. Fig. 4 (Plate I) shows similar curves for C and D. Figs. 3 and 4 show similar curves for A and B and for C and D, but at the deep load line assumed, the same in all cases. Fig. 3 shows similar curves A and B enlarged to A's displacement. Fig. 4 shows similar curves for D and for C enlarged to D's displacement. Figs. 5 and 6 (Plate II) give similar curves showing the longitudinal distribution of the internal capacity of the ships.

These have been included, as they afford a ready means of comparing the forms of the ships, and it is thought that they may be interesting.

NOTE.—The metacentric diagram of steamer E is given to show that a very considerable increase may be made in the beam without raising the metacenter much, provided that the ends of the ship be fined.

DISCUSSION.

Mr. W. RICHARDSON. Might I just ask a question for information as to this very interesting paper? By reference to the paper Mr. Biles will see there are 15 knots to steamers A and B, the one with over 5,000 and the other with under 4,000 horse-power. In another table the revolutions at the same speeds seem to be almost precisely the same. The question I wish to ask for information is, were the cylinders of one ship much larger than in the other? The indicated horse-power in the one is very much larger than that in the other, and the revolutions are the

same. Therefore I want to know, were the cylinders greater to give that indicated horse-power or was it got in some other way?

Mr. JAMES HAMILTON, Jr. At the very threshold of this paper the author seems to find it necessary to disclaim having been led away by the comparative resistances of long ships made by Mr. Froude. He seems to find it necessary to disclaim having made the mistake that many of our friends have made in working from those experiments. I think the experiments (it is no fault of Mr. Froude, it is the fault of the people who fall into the error) gave the length and breadth of ships, but I think there was no coefficient or any means by which one could readily discover what the forms of those ships were. The forms of those ships were excessively fine that Mr. Froude tested, and I believe many people have fallen into the error of having increased the proportion of breadth to length of ship without having taken care to fine the ends. I think Mr. Biles is to be congratulated that he has not fallen into this error; that he has erred on the safe side; because I hold that this ship B, and in the other example the broad ship D, as compared with C, are, in the true sense of the word, finer ships than the other ships are—considerably finer. Of course, Mr. Biles may not agree with me in that, and there may be many gentlemen who may not agree with me, but it depends altogether upon the measure that we are to apply to fineness and fullness of a ship. Model experiments alone, I believe, will determine properly what is the proper measure of fineness and fullness, and, I suppose, it comes to this, that the ship that goes with the least resistance is the finer ship, and the ship that has most resistance is the full ship, that is, in dealing with different proportions of length to breadth and fineness of form. In the absence of model experiments, I have been in the habit of taking the breadth plus the draught of water multiplied by a coefficient—the coefficient of fineness; and I find that this measure agrees with those four vessels given by Mr. Biles, if taken at what might be regarded as the full speed of those ships, something like 14 or 15 knots, very closely, and measure the fineness or the fullness of those ships; in fact, the resistance, I think, follows within 7 or 8 per cent. of that measure. Do not misunderstand me. I do not put this forward as a measure to be put in competition with the Admiralty formula, because the question of speed does not come into it at all. It might take the place of $D^{\frac{2}{3}}$ perhaps, or the area of the midship section, but it is simply a measure for dealing with ships when they are put into the shape that Mr. Biles has put them into, when the ships are about the same lengths and when the speeds and displacements exactly correspond. Now, I have put these vessels into that shape, and I find that A would be represented by 38.89 and B by 39. Therefore, I think B is to all intents and purposes the same as A, and we would expect that she would take the same horse-power to drive her, but she did take less horse-power. In the same way C is represented by 35.3 and D by 32.26, so that D, measured in that way, is a finer ship than C. By these figures, taken as the rule of

three, the displacement of A being 5,130, the horse-power would be 5,140, whereas the actual horse-power, I think, was 3,940. I do not agree with Mr. Biles, and I think he has some hesitation himself in saying that a ship that has been lying in the Clyde for five months would have no greater resistance than one that has been lying only three weeks. In the only direct experiment that we had occasion to make, we found that after having taken a vessel upon a slip and painted her bottom, we really got 20 per cent. more speed. I am quite sure there would be a very considerable difference in the resistance between a ship that had been lying in the Clyde five months and one that had been lying only three weeks. At the same time, even when we have made this allowance, it seems there is a fair balance in favor of the broad ship when she is fined to the same extent that this vessel has been. I do not know that the four examples, though they are very valuable, are sufficient to enable us to state accurately that the resistances of broad ships and fine ships would be quite as represented in this paper, but I think the greatest credit is due to Messrs. Thomson for having put the results of four such large ships as that before us. Few firms can put such an array of data as that of four large steamers before us, and I think they deserve the greatest credit, and Mr. Biles also for having worked it out.

Mr. J. INGLIS, Jr. My lord, I think the title of this paper might have been better worded; it might have been, "Upon the disadvantages of increased proportion of length to beam in steamships," for this reason, that the increased beam can scarcely be called an innovation so much as a return to the traditions of our elders, which we have perhaps discarded a little too readily. I do not think it is necessary to go back to the book of Genesis for the specification of a very old vessel, which was perhaps not formed so much for speed as for seaworthy qualities; but if we only go back as far as 1866, to the transactions of this Institution for that year, we find there that it was gravely argued whether a vessel was not made unseaworthy by being made eight beams in length. And a very distinguished member of this Institution, Sir Edward Reed, I remember deprecated the idea that it should go forth to the world as the opinion of this institution that the London foundered because she was excessively long; because he, for his part, did not think she was an excessively long vessel. The London was under seven and a half beams, so that the advantage of beam seems to have been recognized even in those days as a good thing for seaworthiness. So far as regards the advantage in some cases of an increase of beam for speed, my own observation quite corroborates what Mr. Biles says, only I think that the dimensions of an ordinary merchant ship have not been ten beams in length for so very long as to give that proportion the authority of antiquity. It is quite within the recollection of all of us when a vessel of ten beams was considered almost a monstrosity in length. Mr. Biles has certainly done very good service in pointing out that even more moderate lengths might be too long for purposes of speed, and that the

increase of beam of a vessel of even less than eight beams in length might be attended with advantage. I remember in 1865 we were asked to construct two small paddle-steamers for the river Clyde, and we made an experiment then which was very interesting, and the result was the exact opposite of what all the wise people in the neighborhood predicted. We built one vessel to 195 feet in length by 22 feet in breadth. The other vessel was 190 feet long by 23 feet in breadth, that is to say, 5 feet shorter and a foot broader; the displacement was the same; the engines, boilers, paddle-wheels, and everything exactly the same, and the shorter vessel went fully a knot faster than the longer one. Of course, her draught of water was less. But the displacement was the same, and the advantage in speed was what I have said. Another point is this. Mr. Biles, of course, knows perfectly well—as well as anybody, and I do not think he has laid sufficient stress upon it—that in talking of the advantage of beam and fine ends he is speaking of those fast mail steamers that Messrs. Thomson have built so many of. I do not think he would claim so much advantage for his proposal in the case of merchant vessels to carry heavy cargoes at low speeds; in fact, if you look at the tables of the vessels A and B, as far down as 12 knots, the long vessel has rather the advantage, and I think it certain that at speeds such as 8 or 9 knots the long vessel would have very much the advantage. If I am not mistaken, that was pointed out by Mr. Froude in some of his papers—that, on account of the skin friction being the most important element in the resistance at low speeds, the effect of form was not so marked in the results as the effect of the diminution of the amount of skin, which perhaps could be made less in a long parallel-sided vessel. The comparisons I have made at any rate lead me to the conclusion that for the heavy weight-carrying ship of a low speed a broad vessel with fine ends is not the best form.

Mr. R. E. Froude. My lord and gentlemen, I did not quite understand Mr. Hamilton's reference to my father's paper on the comparative resistance of long and short ships. I understood Mr. Hamilton to say that there was nothing in that paper to indicate the peculiarity of form which was recommended to accompany the increase of breadth. I understood him to say that increase of breadth was recommended, but that increase of fineness of ends was not laid stress upon. I therefore wish to point out that the lines of the forms which were compared were on the diagrams accompanying the paper and printed in the Transactions. Perhaps I misunderstood Mr. Hamilton's meaning, but I think the paper sufficiently made clear the characteristics and differences of form upon which the gain in the performance depended. In reference to the comparison between the ships A and B, Mr. Biles gave reasons for supposing that the gain in the performance in the ship B was not due, or was not by any means entirely due, to any excessive resistance due to friction of skin in the form A. Mr. Hamilton contests this, and says that he thinks the ship certainly should have been foul if she

remained in the water so long. There certainly seems some reason in that, but there is in the internal evidence afforded by the results ground for supposing that the friction was not excessive in the ship A, in the fact that at the lower speed the performance of the ship A is the best of the two, and as skin friction is most important of all at low speeds I think that certainly the ship cannot have had any very great excess of skin friction. I certainly feel myself personally very greatful to Mr. Biles and Messrs. Thomson for doing so much to introduce the principle which was advocated by my father in that paper into practice in the mercantile marine. I should wish, however, to point out one qualification in particular which my father introduced in advocating that principle, which was this. Mr. Biles is here dealing with results of trials in smooth water. Now, it is certainly the case that a diminution of the fullness of the ends and concentrating the displacement in the middle of the ship and removing it from the end is certainly likely to make the ship pitch, and it is not only objectionable on that ground, but the performance of such a ship in a seaway would, from that reason, be comparatively less favorable than in still water, because the pitching must certainly rather tend to increase the resistance. So that it is probable the gain in the performance which we find in these trials to be realized by ships with fine ends in still water is greater than they would evince in practically working at sea.

Mr. W. JOHN. My lord and gentlemen, when we compare the differences between A and B ships and C and D ships, I would like to draw attention to this one fact, that in A and B the propellers are almost identically alike, and there is not such a very large difference between the horse-power for the particular speeds, although the increase of beam is nearly as great in the one case as in the other. But in the comparison between A and B there is a difference of 6 inches in the diameter of the propeller, 6 inches in the pitch, and while the diameter and pitch are reduced, the surface is increased by 10 feet. I do not know whether there was any difference (perhaps Mr. Biles can tell me as I go on) in the immersion of the screw in those two ships. Were they at about the same draught?

Mr. J. H. BILES. They were both well immersed, I think. One was immersed about 18 inches, and the other probably 22 or 24; they were both well immersed. For all practical purposes they were practically the same in immersion, I think.

Mr. JOHN. I think it is quite possible. While I entirely agree with Mr. Biles' idea, following Mr. Froude's ideas, and following ideas that prevailed even before Mr. Froude demonstrated it, that, by increasing the beam of ships and fining the ends, you can get better speed with the same power, I have had a little experience lately which induces me to think that there may be even more in those little differences in the propellers of those two ships A and B than even in the foulness of the bottom or in the difference of skin friction. I will give you a couple of ex-

amples with regard to two sister ships that I have had on trial comparatively recently. For experimental purposes the screws and the spare screws were made slightly different. There was not more than a difference of 6 inches in the diameter, and very little more difference in the pitch than that. We put on our favorite screw at first, because we thought we should develop the power and get the speed easier. And we did not within about a quarter of a knot. Then we put on the coarser screw, and we got scarcely such a good result. We just tipped the ship up, altered the trim a foot, which would bring the screw 6 inches more out of water, and I must say it was purely to my mind a toss-up whether we should improve—I thought we should improve matters, but I will defy any one to prove it beforehand. The result was that we got nearly half a knot more speed, the ship's bottom exactly the same and the ship exactly the same. We had almost an identical experience with the sister ship within a couple of months after that. Therefore, I am sorry that there is so much difference between the screws of those two ships, because I think it is quite possible that the difference may arise from that quite as much as it might from either of the other causes assigned here. There is only one other remark I would make. I think, now we have started increasing the beams of our ships and fining the ends, we shall probably go on until we are brought up by the one simple question of stability—that we shall go on increasing the beams, we shall get better performances, until we get the ships so stiff that they will begin to roll and become uneasy ships at sea, and then we shall begin a retrograde movement, and the sooner we can realize that position and steady our oscillations and not go too far in the one direction or the other, the better it will be for the mercantile marine.

Mr. E. Withy. My lord, I should like to point out a difference between the two ships A and B. If you refer to the first table on page 155 there is a difference of draught of 2.1 feet. I should like to ask Mr. Biles if he can give us the figures of the displacement of those two ships at those respective draughts. It will be a valuable addition to the paper, I think. I presume that the co-efficients are taken at the draughts given. Perhaps he could confirm that. Then I should like to point out that with regard to the indicated horse-power, it is identical for both ships at 13 knots and is more for the broad ship at 12 knots. Perhaps Mr. Biles will give us some explanation of that, or some theory with regard to it.

Mr. J. H. Biles. My lord, in replying to these questions I will take them in order, if you will allow me. Mr. Richardson has asked whether the engines were the same. The volume of the cylinders were about the same in the two cases, but in the case of B, that is a three-cylinder engine, and A is a two-cylinder engine. The capacity of the boilers is a little more in the case of A than in the case of B, but the pressure at which the boilers were working was considerably more in the

case of A than in the case of B—about 15 pounds mean pressure. With respect to Mr. Hamilton's remarks about the measure of the fineness of a ship, I have had the pleasure of reading a paper he read at an institution in Scotland on a similar subject to this, and in which he explained that; but I have not been able thoroughly to appreciate the value of this coefficient. I do not care to say anything more about it at present, because I hope, before Mr. Hamilton's paper comes on to-morrow night, to be able to get a little appreciation of it, and also to be able then perhaps to say whether, in my humble opinion, the thing is of as much value as Mr. Hamilton thinks it is. I have some data worked up in connection with it, but I have never been able to properly digest them in order to give anything like a definite opinion on the question. Perhaps, my lord, you will allow me to ask Mr. Hamilton a question?

The PRESIDENT. By all means.

Mr. BILES. He says he tried a vessel on the Clyde. He tried it before docking and after docking, and got a difference of 20 per cent. in the speed. I think there is a little error in that. If there is not, I should like to know what the absolute speed was.

Mr. HAMILTON. Did I say 20 per cent.?

Mr. BILES. Yes.

Mr. HAMILTON. The actual speed before docking was 10½, and we got half a knot more. Of course that is a very different thing. I withdraw the 20 per cent.

Mr. BILES. That is about 5 per cent. on the speed, which probably means about 15 per cent. on the horse-power. Assuming Mr. Hamilton's figure of 15 per cent., instead of taking the figure I gave, 6½, still leaves the broad ship 15 per cent. to the good; 30 per cent., as I have shown, is probably considerably on the wrong side; that would still leave it 15 per cent. better. I would make it compare more favorably with the cases C and D, as given where the difference is given as 10 per cent. I do not wish to claim any credit for proposing this increase of beam. As Mr. Inglis says, you want to go back as far as the Book of Genesis before we get to the person who deserves the real credit for the particular proportions. But though I do not wish to bring in the Book of Genesis I did not intend this paper to be a Book of Revelations. Mr. Inglis has said with regard to the question of narrow ships, that the narrow ship is the better ship for slow speeds. Mr. Hamilton—I am sorry to drag him into the discussion again, but he has worked up some very valuable data in connection with that, and I must leave him to reply to that remark in his paper to-morrow night, and I think he will be able to lay some data before Mr. Inglis for his consideration which may alter his opinion. I am very much obliged to Mr. Froude for the commendation he gave with respect to the paper. With regard to Mr. John's criticism, I thoroughly appreciate the difficulty of comparing results of trial trips, as I have mentioned in the paper. I have

To Il

XIII.

A SELF-PROPELLING, SELF-CAREENING FLOATING DOCK.

By G. B. Rennie, Esq., *Member of Council.*

[Read at the twenty-fourth session of the Institution of Naval Architects, 16th March, 1883; the Right Hon. the Earl of Ravensworth, president, in the chair.]

On the 18th March, 1869, I had the honor of reading a paper before this Institution "On the iron floating dock of Carthagena, its proportion and relative stability," in which, after describing the capabilities of this dock, I gave the results of certain experiments on the stability of different sectional forms of floating docks, with and without the weight of ships on them. Suggestions were also given as to an improved form of dock for being towed across the Atlantic. The president (then Sir John Pakington) made this remark in winding up the discussion: "I think we are much obliged to Mr. Rennie for the able paper he has given us, combined with his very important practical experience. Although I am afraid it does not bear much upon the question of conveying docks to their ultimate destination, &c." And again, a year later on, he says, "Every naval man who hears me will concur in the opinion that nobody could contribute a more valuable addition to naval contrivances than to invent a really efficient floating dock, and not only a floating dock, but a dock which, when afloat, may go to any part of the world."

I trust, therefore, the above-mentioned remarks will be sufficient reason for the paper before us.

A dock, to do what is required, must not only be capable of being propelled, but must have ready means of cleaning the under-water parts, in order to insure the speed being maintained by preventing undue resistance, besides all the conveniences of docking ships with safety and expedition. As far as an efficient floating dock for docking ships, this has been solved by the systems adopted both at Carthagena and Bermuda. The cleaning of the under-water parts in both these examples has been successfully accomplished, each in a different manner; but the self-propulsion of a dock combining these qualities has yet to be done.

I propose to accomplish this—

1st. By making the dock of suitable form and strength, so that ships may be easily docked without the use of either gates or caissons (as at

Carthagena), and of a form easily propelled at a moderate speed through the water.

2d. To make the pumping machinery for emptying the dock also serve the purpose for propelling it through the water.

3d. By giving the dock such a sectional form, that by means of water ballast it may be careened over first one side and then the other up to the keel line.

No. 1 may be best explained by reference to the drawing and model.

No. 2. The means proposed for this is utilizing the machinery for pumping out the dock for propelling it, by means of the water discharged in the contrary direction to which the dock is to be propelled.

The example best known of this system of propulsion is that of H. M. S. Waterwitch, but which, compared to similar ships, such as the Viper and Vixen, propelled by twin screws, did not give such a good result of speed for power.

Thus, the Waterwitch gave only 9.3 knots, with 760 i.h.p., whereas the Viper gave 9.6 knots, with 696 i.h.p. This difference seems to me to be fully accounted for by the small propelling area of the water jets, only about 5⅓ square feet in the Waterwitch, whereas the disc area of the two screw propellers was 127 square feet in the Viper, or as 1 : 24.

Mr. White, in his "Manual of Naval Architecture," discusses the whole question very clearly, and with his views generally in the matter of water-jet propulsion I quite agree.

In ships of the usual construction the water-jet propellers seem to be difficult of application, in consequence of the large inlets and outlets that would be required for the passage of the water to give a good result as compared with screws or paddles. In the case, however, of a floating dock the case is quite different. Here large pumping power is required for emptying the different compartments, and this can be distributed along the length of the dock on either side, and although the power for emptying the different compartments of the dock would not be required to the extent necessary for propulsion even at a very moderate speed, there would be no disadvantage in having a surplus power for that purpose. The maximum speed that seems to me to be necessary for a floating dock to be propelled to its destination, or from port to port, should not exceed five knots, and probably four knots would be found sufficient. The propelling power for such a speed, with the necessary propelling area, such as would be considered a proper proportion if the paddle or screw were adopted, could easily be distributed in several sets of pumps and engines, with separate suctions and discharges, on either side of the dock; the area, pressure, and velocity of the discharged water being made in proportion to the propelling speed required. In the drawing shown, which is for a dock 350 feet in length, and suitable for ships of 4,000 tons' weight, six sets of engines, pumps, and discharging nozzles are shown on each side, which may be compared in their disposition to the oars or paddles of a boat or canoe.

3. The careening of the dock would be effected by utilizing the pumping machinery for filling the water-ballast chambers on either side. The engines would then be stopped and all made secure for careening, when the sluice valves on the side to be raised out of water would be opened, the dock would gradually heel over, and assume the position shown in Plate I. The form of the section, the center of gravity of weights, and the weight of water ballast have all to be carefully considered and calculated in order that the dock may be careened over, so as to expose the under-water part up to the keel. When one side is done the same operation can be performed for the other side.

In conclusion, it seems that the necessity of dry-dock accommodation is increasing all over the world in proportion to the increase of ships both naval and mercantile, and the more general adoption of iron or steel in their construction. There are many places where masonry dry docks are practically impossible to construct, and the time and cost to construct them in other places is a bar to doing so. That patent hauling-up slips are only suitable for small vessels under 2,000 tons. That to re-erect abroad or tow a floating dock to its destination is often a most difficult and expensive matter. That the want of dry-dock accommodation would be fulfilled by making it self-propelling and navigable. That the balance of advantages of doing this is, propulsion by means of water jets as described. That such a dock, having all the maritime rights of other ships, would be able to enter and anchor in many ports, and there dock ships without connection with the shore; and although, perhaps, the speed of five knots might prevent it accompanying a fleet, it might meet it at the port of rendezvous and be ready to examine and clean the under-water parts of the fast cruisers and other suitable ships.

DISCUSSION.

Mr. LIGGINS. My lord, I think this is a very valuable paper, and a most sensible and practical suggestion. I make that remark because I have had some little experience among the different West Indian Islands of the great inconvenience occasioned, and sometimes the abandonment of ships, in consequence of there being no dock to take them, or any means of getting any disabled ship to the only dock in the West Indies, situated at St. Thomas. If St. Thomas dock had the power of moving to another island, it might probably be the means of saving for future use a ship of 3,000 or 4,000 tons. The allusion to the Waterwitch in the sense in which reference was made was all very well, but the Waterwitch was a comparative failure, because she, for speed purposes, did not succeed in attaining her speed in the same economical way that screw ships do; but still, at the same time, that is no reason this dock should not go at a slower pace, even if at a little extra cost, than she might do if propelled by a screw. Therefore water propulsion is a very

nice and ingenious adaptation of the machinery which she has on board for pumping purposes, and that, it seems to me, would act efficiently at the slow rate of speed at which it was necessary to go through the water. I think, again, that the same application of water-propelling force is most ingeniously applied in the way Mr. Rennie proposes to careen the vessel, and enable him to do those necessary repairs in painting and cleaning the bottom which are so essential to the preservation of all constructions of iron. I think, therefore, my lord, that this is particularly, in a mercantile point of view, a very valuable invention, which deserves to be tried on a practical scale.

Vice-Admiral DE HORSEY. My lord, I think Mr. Rennie would add very much to the interest of this paper if, in his reply, he would give us a few particulars as to the sizes of the dock, and, within £20,000 or £30,000 or so, of its cost.

Mr. F. K. BARNES. My lord, I was in hopes that some gentleman more capable than myself would say something in regard to this dock. I do not know whether the dock would be a paying speculation or not, and that, it seems to me, would be a very important point to ascertain; but, supposing that it were desirable to build a dock to effect the object aimed at, I feel quite sure that this plan would meet the requirements in the most economical manner possible. Large pumping power must be provided for cleaning the dock. That power Mr. Rennie has utilized to propel it along, and I think there is, so far as any one can see, from looking at it, and without going into the details, every reason to believe that he would get a fair speed—a speed sufficient to push her along. He names a very moderate speed of four knots, and I should think that would be quite attainable. I was under the impression that the area of the nozzle of the Waterwitch was about 7 feet; perhaps Mr. Rennie could say?

Mr. RENNIE. It is $5\frac{1}{3}$ feet.

Mr. BARNES. I was under the impression that it was 7 feet, but I should think from the areas of nozzle that are obtainable—in fact, you could get very large nozzles here if you please, without increase of resistance—the speed would be exceeded. The method of working the dock, which is very similar to the dock at Bermuda, really leaves nothing further to be desired. The dock can be inclined by the large pumping power which pumps water into the upper compartments already provided for it. When inclined the under-water parts can be cleared and overhauled, and I think when it is made perfectly manageable it would be very economical, and the clearing could be done once a year without any difficulty. It is essential, whatever dock you have, that it should admit of being cleaned so that it might be properly taken care of. That is the more necessary in this case if you propose to move from one port to another. Whether or not it would pay, I am not quite clear. Of course the ports at which a dock like this could be used in war time would be limited. I believe it could only be used in our own ports or

the ports of our allies, and all other ports would be blocked to it. At the same time I think, under certain circumstances, it would be of immense value for warlike purposes. I have nothing more to say, but that I think the dock and its working are quite practicable, and I should think it is about the most economical mode of effecting the objects aimed at that could be adopted.

Mr. STANDFIELD. My lord, I should like to make a few remarks on the subject of this dock which is now before us; more particularly in regard to the careening for the repairs of the dock itself. It has just been stated that it appears to be an efficient way of getting to the bottom of the dock by careening it over in this way (illustrating), and that it is like the Bermuda dock. I believe the Bermuda dock has been erected upwards of fifteen years, and that they have careened it only once or twice in that time, because the process is so very dangerous; and I think, if I am any judge in this matter, that the dock shown on this diagram could not be careened so as to get at the center as shown on this diagram, and that it would be a very dangerous process, as the author of the paper remarks, and would require great care and great knowledge of the laws of stability and displacement before it could be attempted. I think that a proof that it would be a very dangerous dock is, that the Bermuda dock has been careened only once, I believe, in that time. Mr. Barnes can tell us whether that is so, and how long it has to go out of use to be prepared for that careening. If it has to go out of use for a long time, several days, or a week, it is a serious matter, and I believe that is the case at Bermuda. I believe it takes a considerable time to prepare for it. My partner, Mr. Latimer Clark, read a paper here some time ago, showing a dock that could be docked in two hours on an even keel, and which would not have to run any of the hazards and dangers of careening, which there are in the case of this and of the Bermuda dock. It is evident if careening were a desirable thing, that vessels themselves might be careened. Now, there are several docks in existence which have never been careened, and their bottoms have never been examined, because there are no ready means of doing it. Again, with regard to propelling, it is quite evident that the engine power necessary to pump out the dock—even to pump it out in half the time that any dock was ever yet pumped out in—would want quadrupling to propel it at anything like five knots an hour. If the same engine power were put on the screw propeller the shafts that lead to the chamber would be made to gear to the screw propeller, and could propel the vessel twice as fast as it could possibly be done with water jets. I do not think that water power is to be thought of for a moment for the purposes of a sea-going dock. There is no doubt that, for the purposes of a sea-going dock, twin screws would be much more efficient than water propulsion. The possibility of careening such a dock is very doubtful; the sides should be higher and the dock more of the shape of the Bermuda dock to be careened. I do not consider this dock as nearly as good in shape as the

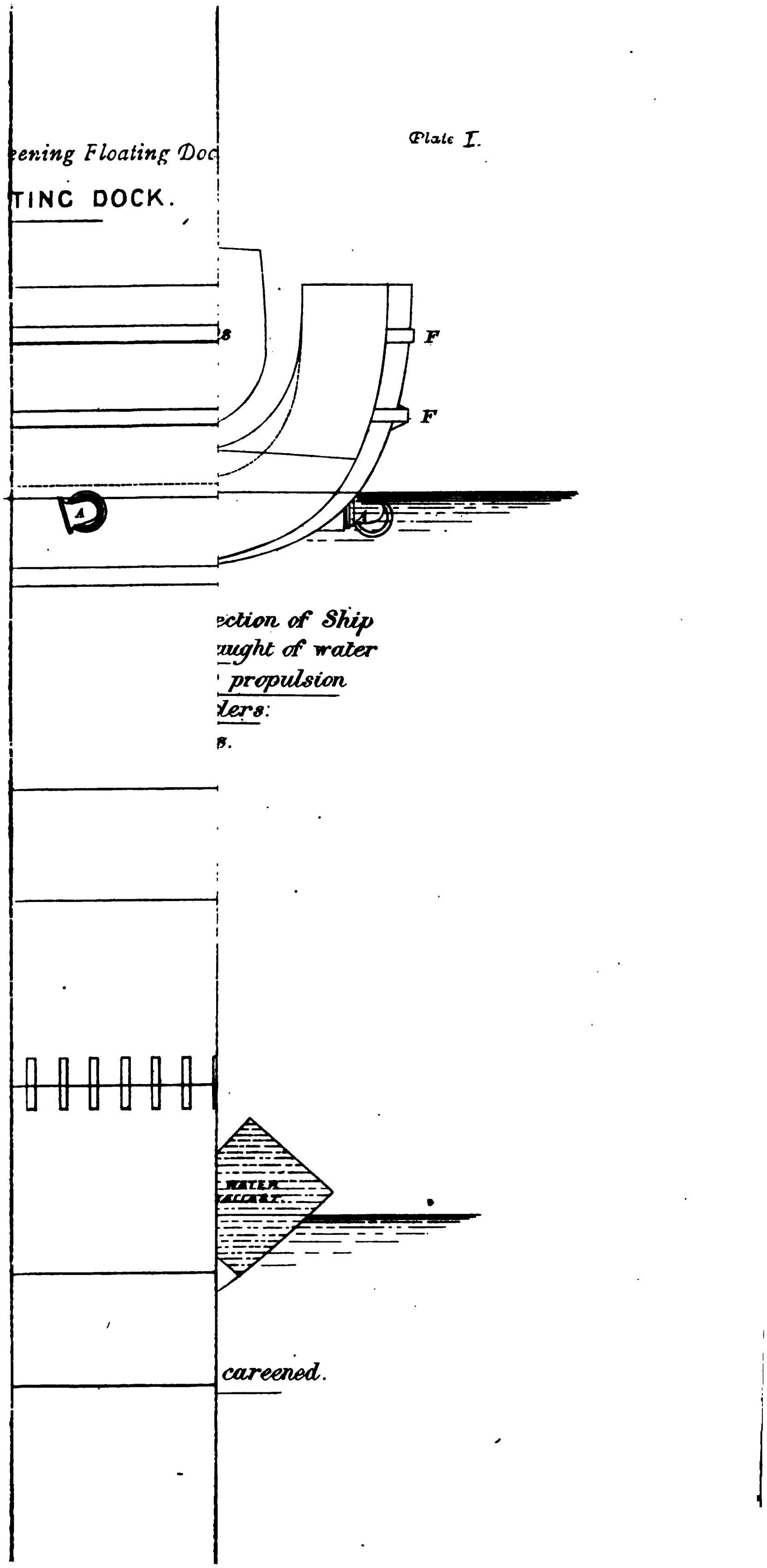
Bermuda dock for careening; this dock is shown as being provided with long projecting ends similar to our self-docking dock, but as these ends have no corresponding portion of sides they must necessarily deprive a dock of this shape of much of its careening power. A dock that requires careening would not be anything like such good value for money as a dock that could lift its sides and bottom in turn out of the water on an even keel whenever necessary for cleansing and painting. It must, moreover, be borne in mind that the sides of this dock could not in any circumstances be brought into action to increase the lifting power of the dock, as is the case with the self-docking floating dock.

Mr. G. B. Rennie. My lord and gentlemen, I have to thank Mr. Liggins and Mr. Barnes for the kind way in which they have said that the dock, as far as they have seen the thing, is a practical and economical and useful apparatus. Admiral de Horsey asked me a question about the size and cost. I give the approximate size in the paper, viz, it was for vessels of 4,000 tons and 350 feet long. It would lift a vessel such as the fast cruisers that are now being made, that is, vessels of 4,000 tons weight and 350 feet long could be easily docked in a dock of the size there mentioned. Mr. Standfield seems to think that careening is a very dangerous matter; in fact, if I understood him rightly, he said also that I said so in my paper. I think, if he reads my paper, he will see that that is not what I say. I said that the calculations of the weights, and the form of the dock, and the center of gravity had to be carefully made in designing the dock—not careening it, there is little difficulty in doing that with safety. Then he thinks, also, that in consequence of the Bermuda dock having been docked only once or twice, and the operation being very slow, it was a very dangerous and undesirable thing to do in any future docks. How many times the Bermuda dock has been docked I do not know. I believe the operation is very slow—that it is not from the danger of doing it that it has been done so little, but it is from the fact that the pumping, or the means of filling these chambers, is a very slow process, and it takes a long time to do it. The exact time I do not know, but here, in the case before us, the difficulty is got over by having very large pumping power, which fills the chambers in a very short time. I calculate that the careening ought to be done very easily, so far as the pumping is concerned; it could be pumped up in half an hour. I do not presume that the water could be run out and the dock cleaned in that time, but the dock heeled over on one side, which might be cleaned, and then the same operation would have to go on for the other side. Then with reference to what Mr. Standfield said about screws. He believes the screw propeller would be a far more advisable propeller than the water-jet propeller. He may know more about that than I do, but, as far as my calculations go, I think it would be very difficult to apply screw propellers. I do not know how, in a dock of such a form as proposed, you could put a screw propeller advantageously, and if you have the same propelling area in the

form of the water-jet, or screw, or any form you like, you have the same means of propelling with the same pressure, and it seems to me you are likely to get the same speed with the same propelling power. There may be some little difference in friction, but I doubt whether at the slow speed there would be. Then there has been mentioned the case of the Waterwitch, where it was compared with a similar vessel; the speed compared to the power was less in the Waterwitch than in her sister vessel propelled by twin screws, as mentioned in the paper. To anybody who goes into the matter, I think it is clear that the reason of that was the small size of the jets of water which propelled the vessel along. This view is confirmed on referring to the propelling areas. In some of the trials made in the Waterwitch the area was reduced, and when the area was reduced for the same power the speed fell off considerably. The converse of that is this: That if you increase the area, taking no other view than that, the chances are that you would have got a higher result rather than a less result in power compared to speed. Mr. Standfield also mentioned the question of lifting the dock with a flat bottom as being preferable to careening. No doubt if you have a flat bottom dock it is, but there are two or three ways of doing it. Mr. Standfield has one plan. The plan we adopted at Carthagena is shown on those photographs before us. That dock was built first of all in a shallow basin. The use of the basin was enlarged afterwards, so that now we have a further use for the basin, that is, the dock will go with the vessel on it into this basin, and a vessel can be hauled in a horizontal line from the dock to the ship, so that the same floating dock for lifting is available for seven ships of 300 feet long each. I do not know that there is anything else to say, except to thank you for the kind manner in which you have received my paper.

The President. It only remains for me to thank Mr. Rennie, on your behalf, for his very valuable paper.

Plate I.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